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THE STREAM OF LIFE.

AMONG the results of modern, nay, of quite recent, scientific research, I know none more impressive than the recognition of the stream of life, animal and vegetable, which has flowed over the earth during millions of past years. Starting we know not how, coming we know not whence, the stream of life has left traces of its existence in all the rock strata of the earth from the palæozoic onward through the secondary and tertiary systems, onward to the pleistocene and to those strata which are called recent, though some of them, measuring their duration as men measure time in considering the progress of races and of nations, seem of vast antiquity.

Let us consider how the history runs, not entering into details, but taking such a survey as shall bring the broad features of the scene before us, and show its cosmical rather than its merely terrestrial aspect.

How first the earth's crust was formed we know not, for the traces of the first formative processes have long since been swept away. Probably each portion was formed again and again, solidifying and melting, and resolidifying and re-melting, many times over before it finally assumed the solid condition. In some cases the solid part of the crust, being of greater specific gravity than the molten, would sink as fast as formed, to melt again, and again return to the surface, not finally solidifying in a permanent way until many hundreds of thousands, perhaps millions, of years had passed.

Whether any part of the present crust of the earth can be regarded as representing the solid crust fashioned when first these alternations were completed may well be doubted. The so-called Archæan rocks have been supposed by some to be the primeval crust of the planet. But others regard them as belonging to a much later stage. And others yet admit that we cannot tell what interval separated the formation of these, the most ancient rocks of the present earth, from the first formation of a crust upon that region of the earth where the Archæan rocks are found. But this, at any rate, is certain: Whenever the Archæan rocks, which have been exposed in various parts of the earth by long continued processes of denudation, were actually formed, they have mostly (if not all) undergone great changes during the time that they have been buried beneath strata formed later. Moreover, it must be remembered that the lower primary rocks which were originally deposited on these Archæan rocks were formed from them by processes of denudation, and that, therefore, we cannot expect to find anywhere the ancient face of the first-formed crust. Indeed, denudation in those remote times, certainly 20,000,000, and probably 100,000,000 years ago, was a very much more active process than denudation as it takes place now. For the waters of the ocean were greater by one-half in quantity, were intensely hot, were loaded with destructive acids, were more actively moved; while the air was not the pure life-nourishing air we breathe now, but an atmosphere whose breath was fire, laden with destructive vapours, swept by tremendous storms, and bearing clouds from whose bosom descended torrents of hot water, mixed with sulphuric acid, boracic acid, and other powerfully acting liquids. By these destructive agencies the first formed crust of the earth must have been rapidly denuded, and fresh layers formed at the lower levels, to be raised while the higher levels were depressed, fresh denudation following, and fresh layers being formed, probably during millions of years, before life was possible upon the earth.

It appears to me, indeed, that in the existence of fossil traces of life, animal and vegetable, in the lowest of the palæozoic rocks, we have decisive evidence that immense periods of time must have separated the formation of the rocks next below them from the first formation of the earth's solid crust. For it is certain that during millions of years following the solidification of the earth's crust, life must have been impossible. Even apart, therefore, from the decision of the much-vexed question whether the formation named *Eozoon* found in the Archæan limestones of Canada is to be regarded as the fossil evidence of a reef-building animal (a Foraminifer), which would throw back still farther the time of the first solidification of the crust, we may safely conclude that the Archæan rocks represent formations which had no existence with their present structure until millions of years after the earth first had a solid surface.

With our present ideas respecting the laws of biological evolution (ideas based, be it remembered, on observed facts) we may deduce the same conclusion from the character of the fossils found in the lowest of the primary or palæozoic rocks. We know that whatever may have been the actual beginnings of animal and vegetable life on the earth, the flora and fauna even of the lowest palæozoic strata cannot have come into existence save as the product of immense periods of past time. In the Lower Silurian rocks, indeed, we have but few fossil traces of the vegetation of the remote time when they were formed; but this is chiefly due to the fact that the Lower Silurian rocks are chiefly of marine formation. But the animal life, even of the oldest stratified rocks, suffices to decide the question of duration in the most emphatic manner. Of course, the record is incomplete; but in its very incompleteness it is the more decisive. We find complex and advanced forms, while the simpler forms, which necessarily existed at the same time, have left no trace of their existence. We can understand, then, how all forms of life, simple and complex, animal and vegetable, belonging to earlier strata, have disappeared, leaving no fossil evidence. And the positive evidence given by the fossils we do find, while thus giving to the negative evidence its true meaning, has its own definite and unmistakable value.

If we consider, for example, but a single class—say the Trilobites (now extinct, though transiently represented in the early life stages of the King Crab)—we have as decisive evidence of enormous preceding time-intervals as if we had fossils of 10,000 species of Silurian life, animal and vegetable. Those strange crustaceans existed at the remotest time to which geological history looks back, in many different forms. Each had a strong head-shield to protect its head, a smaller tail-shield, and a ringed body which could be so curled that the two shields came together, and the whole body was protected. Some were nearly two feet in length (possibly some were much larger, but I speak

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only of existent fossil evidence), and from this size they ranged down to minute forms, such as the *Agnostus Princeps*, which was less than half an inch long. The number of rings also varied, the *Agnostus* having but two, the *Microdiscus* four, the *Erinnys* no fewer than twenty-four. Still more striking evidence of development, requiring hundreds of thousands of years for its full work, is found in the complex eyes of the more advanced species of trilobites, for while the *Agnostus* was blind, most of the trilobites had eyes, some with fourteen facets, others with as many as 15,000! All through the Silurian period these crustaceans continued to thrive and multiply and develop into varied forms. But as the millions of years represented by the palæozoic strata passed on, evolution in the race of trilobites resulted in the development of other races better fitted for the changed conditions of life; we find no traces of any trilobites later than the carboniferous age; and in that age we find only four, all of which were small. Doubtless many others existed of which no fossil traces have been left. Doubtless, also, trilobites continued to much later times than the fossil evidence attests.

We cannot trace the series of crustacean races which connect the trilobites with their descendants in secondary, tertiary, and recent ages. We cannot even tell what creatures have descended from the trilobites, excepting here and there a race or so, or along what line the descent travelled. We know only that those races which retained their trilobitic characteristics died out, those only surviving which adapted themselves by changes of structure to surrounding conditions.

But the Silurian rocks (the lowest and most ancient of all rocks known to have been life-bearing) were not without vegetable life, and the fossils telling us of the existence of vegetable life show also that immense periods of life must already have passed before that life began. In the Upper Silurian strata we recognise the spores and stems of flowerless plants (*cryptogams*). Club mosses and ferns were particularly conspicuous in the flora of those palæozoic times. "We can dimly picture the Silurian land," says Geikie, "with its waving thickets of fern, above which lycopod trees raised their fluted and starred stems, threw out their scaly, moss-like branches, and shed their spiky cones." But it is only because the cones and spore cases of the club mosses and the tough tissues of the ferns have been well fitted to withstand destructive agencies, and so have remained in fossil form (some few even of these), that we are able to speak of these as forming characteristic features of the old Silurian thickets. Doubtless many other forms of vegetable life existed, whose traces have long since entirely disappeared. The wonder is, considering the immense remoteness of Silurian time, that any trace whatsoever, either of animal or vegetable life, should have reached our time.

Most strange is it to think that even of the denizens of those ancient thickets some traces have been found. Remains of scorpions were discovered in the Silurian rocks of Sweden, Scotland, and the United States almost simultaneously in 1884; while in those of France, also in 1884, the remains of an ancient cockroach were found. Where scorpions and cockroaches abounded, not only must there have been many other forms of land animals, but animal life must have existed on land during hundreds of thousands of years. For the laws of biological evolution do not permit us to believe in the development of living creatures so complicated in structure as the *Arachnidæ* and the *Blattidæ*, save in vast periods of time.

In these early stages of palæozoic time we find evidence not only of multitudinous life, but of many forms of life. Still within the Silurian era we find vertebrate forms in fishes akin to the modern sharks, the sturgeon, and the garpike. (The catfish shares with the cockroach the honour of descent from Silurian times.) Onward through Devonian time vertebrate life presents itself still only among fishes, but now in ever increasing variety and numbers. New forms of crustaceans—the Eurypterids—having affinities also with the Arachnidæ (scorpions), began to replace the trilobites, from which probably they had descended, though after transformations requiring vast periods of time.

Many other forms of life existed also throughout the Devonian period, while an abundant vegetation spread over the land, traces of no fewer than a hundred different species of plants having been discovered in the Old Red Sandstone. True conifers now began to appear, or at least fossil races of such trees are for the first time recognised in Devonian rocks.

The forests must have been uniformly green, for millions of years were to pass before deciduous trees were to appear. Many forms of insect life existed in those overgreen forests; fossil wings of insects have been found which seem even to indicate by their size an exceptional wealth of insect life; for races attain large size only under favourable conditions. Thus among several forms of May Fly we find one whose wings must have had a spread of fully five inches! Strange, be it noted in passing, to find traces thus ancient certainly not less than 20,000,000 of years old, of a creature whose very name – the *Ephemeron* naturalists call it—means the creature of an hour, so short is its actual life, so slight its seeming hold upon existence.

But now for the purpose of the sketch here planned we need no longer consider the details of the progress and development of life. We have thus far considered these because they serve to show how far back, beyond the time even when the oldest rocks were forming, we must throw the beginning of life upon our earth. But so soon as we have recognised this, so soon as we have understood that periods to be measured by hundreds of thousands, if not by millions of years, must have passed while the fauna and flora of even the Silurian rocks were being developed, we can pass onwards over stage after stage of the earth's life, noting how during every stage multitudinous forms of animal and vegetable life existed. Some forms were as yet in the first stages of development, others fully formed, others highly developed, and others already approaching the time when, having outlasted their proper era, they were about to disappear. In disappearing they left the field open to kindred races not descended from them but from the same progenitors, having, however, been saved from extinction by such changes as fitted them for their environment.

In this sense one may compare the various forms of life, animal and vegetable, to the various individual representatives of that form, among whom we recognise at one and the same time the infant, the young, the middle-aged, the old, and those nearing death. For species and genera as well as individuals have their inception, their growth, their full vigour, their decay, and finally their death; though because the periods of time necessary for the full development and the eventual decay of a race are so long that the lifetime of an individual (even of a longer-lasting type) is usually but a second by comparison, we are apt to imagine permanence where in reality there is only relatively long-lasting development and dissolution.

It has been because of this that men have so long been led to imagine that races of plants and animals can be neither developed nor destroyed, that all races must have had their beginning in a process of direct creation, and can only have their ending through a process of catastrophic destruction. Even when geologists began to recognise the enormous duration not only of the so-called primary,

secondary, and tertiary eras, but of the different systems included in each, they still supposed that the fauna and flora of one era were destroyed before another era began, those of the new era being specially created.

At length, however, the great law of biological evolution was discovered, which shows how genera, species, and varieties undergo modifications fitting them for their surroundings, modifications individually small but eventually capable of changing (*when necessary* *) the whole character of the plant or animal so developing—those races which do not duly develop when change is required eventually dying out.

Then science began to see that neither creation nor catastrophe is required to explain the life-history of the earth. Precisely as the geologist sees in processes such as are taking place now the explanation of all those features of the earth, ocean and continent, mountain range and river channel, hills and valleys, ravines and chasms, which in old times had been explained by processes of catastrophic intensity of action, so now the biologist recognises in all the forms of vegetable and animal life, infinitely varied though they are, the product of processes of biological evolution such as we can see in action at this present time, though possibly in the lifetime of a man, or even of a race of men, they may produce scarce discernible changes.

(*To be continued.*)

COLLISIONS AT SEA.

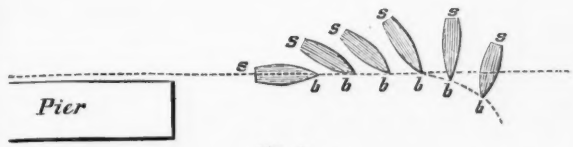
By GILBERT R. FAITH.

ACTION OF THE RUDDER UPON STEAM AND SAILING VESSELS.

MANY years ago I had occasion to make a voyage from Halifax, N.S., to Bermuda. The vessel was the Cunarder *Merlin*, and her commander the "commodore" of the Bermuda, West Indian, and Newfoundland line. We left the pier about midnight, and a very dirty night it was. Something very like a gale from E.N.E. was blowing, with fitful storms of rain. We made a good start, however, and in an hour and a half had steamed past Sambro' light, some twelve or fourteen miles from Halifax, and the dangerous stony-hearted group "The Sisters," with whom mariners care not to have even a bowing acquaintance (no pun intended), were holding their storm-revels not far way. From the time of leaving the wharf I had remained in shelter from the rain under the main companion, smoking and enjoying the scene, if scene could be called what was dimly visible on so dark a night. Suddenly a rush of feet on deck, an excited outcry, and then a tremendous crash on our starboard side. Simultaneously an immense white spectral object loomed high above us, and instantly vanished astern into the darkness, while down upon the stout hatchway, the half dome beneath which I stood, rattled blocks and broken rigging. It was the work of a moment to jump on deck, and of little more to kick off my boots, for I thought from the shock that we must founder, and was determined to make as good a fight of it as possible. A glance round showed that the starboard bulwarks had been swept away from about amidships, and with them boats and a deckhouse, while the starboard stays of the smoke-stack had disappeared, and the main and mizzen shrouds and backstays swung in against the masts. Some minutes passed before I was able to get a word with one of the junior officers, when I learnt that the ship had not sustained any serious damage, and that the injuries she had received were all above board. "What was it that struck us?" I asked. He replied, "A large square-rigged vessel; she bore down straight upon us under as much sail as she could carry; the beggar carried no lights; but just as we struck, I saw a fellow jump into the bows with a lantern." After I had with some difficulty recovered my discarded boots in the darkness, I walked aft on to the quarter-deck and found the captain near the wheel. "A pretty close shave that, Captain S.," I said. "Yes," he answered quietly and seriously, "*but if I had followed the sailing instructions we should not be here now to talk about it.*" It was not my province to ask him to explain, but I felt that at a critical moment his thorough seamanship had inspired him to do something which contravened official orders for such emergencies, but had saved our lives. I have since been enabled to form an opinion as to what the gallant little captain did do, and proceed to relate another incident, which will perhaps enable the reader, if interested, to do so too.

Several years after the adventure narrated above I was standing on the pier at Halifax seeing off some friends on the Cunarder *Alpha*. As the steamer shot out into the stream a friend who stood near me (a shipowner himself, much interested in matters of navigation, and a keen observer to boot) remarked, "G——, what course will that steamer take when her helm is put a-port?" I answered, "Why, surely there can be no doubt about it: her head will come round in the other direction." "No, it will not," he said. "Her course will be very soon changed to that direction; but you will find that it is the stern of the vessel which will be deflected to the left, and not the bow which moves round in the direction you suppose. Now take a line along that ship to the white house on the opposite shore, and note the movement." I did so; and surrendered my opinion at once, for he was right. The following diagrams will illustrate my observation:—No. 1 is the line

No. 1.



No. 2.

which I was confident the ship would follow; No. 2 suggests the deflection which I actually saw.

Now my preconceived idea of the movement of a vessel when the helm is put up or down is, so far as my observation goes, the universal belief. If this is so, what a fruitful source of collisions! In the hope of eliciting a discussion which may throw light upon this important matter and lead to beneficial results, I have written the present article.

* I emphasize this because some opponents of the doctrine of evolution imagine they have fully met the arguments of the believers in evolution, when they have shown that in some cases races have remained unchanged for thousands of years. This may happen—aye, and much longer periods of changelessness—where the conditions have not changed. The Catfish has outlasted tens of thousands of races worthier, it might have seemed, of long continuance.

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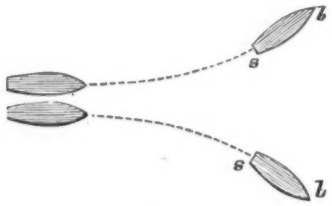
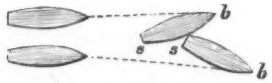
Some ten years ago (if my memory serves) occurred the disastrous collision between the *Grosser Kurfürst* and the *König Wilhelm*. The facts of this catastrophe prove, as it seems to me, the principle above stated, and the ignorance of the commanding officers in regard to it. The two vessels in question had in the course of manœuvring got into dangerous proximity while steaming parallel to each other, and orders were given to sheer off and separate, the belief being that, if one helm were put down and the other up, the vessels would diverge in regular curves. What actually occurred was that the sterns of the two vessels approached each other until the collision occurred, which resulted in the sinking of the weaker vessel, and the drowning of the greater part of her crew. In the inquiry which was made into the cause of this disaster the officers of both ships were pronounced free of blame, and it was determined to institute a course of experiments to ascertain how the vessels could have come into contact while efforts were being made to separate them. I have never heard what the result of the experiments was, and do not think that it was published.

To illustrate by a diagram. No. 1 represents the intended



No. 1.



No. 2.

course of the two ships when separating; No. 2 represents their actual course.

Not long after the preceding catastrophe occurred the following collision, which must be well remembered. Two Sound (Long Island Sound, U.S.A.) steamers of the same line, the *Stonington* and *Narragansett*, were proceeding to their destinations. They had been accustomed to pass each other midway of the route for years, but on this occasion they proved to be approaching each other too directly. The rule of the road was followed on this occasion. Each vessel obeyed the sailing instructions, as was afterwards admitted. If these were adapted to such cases the vessels should have cleared each other. But they were approaching head-on with great speed; there was not sufficient interval to allow for the stern deflection, and they were brought into collision with such violence that one sank with the loss of many lives. The newspapers commented upon this collision as inexplicable because the sunk steamer was struck upon the reverse side to what would have been expected, which fact is easily explained upon my hypothesis. It was, I believe, by ordering the helms of the *Merlin* in a sense directly opposed to the published instructions that Captain S. so deflected the after part of his ship in the few moments allowed to him as to receive a glancing instead of a direct blow.

As a last illustration, I will instance the loss of the fine steamer *Oregon*. The facts in this case are unfortunately not very clearly known, but it is generally admitted that she was sunk by a vessel approaching her obliquely from the port side, which in the fog had got too near to be avoided. It seems to me possible that the collision was owing again to the following of sailing directions, based upon ignorance of the principle I am seeking to establish. Diagram No. 1

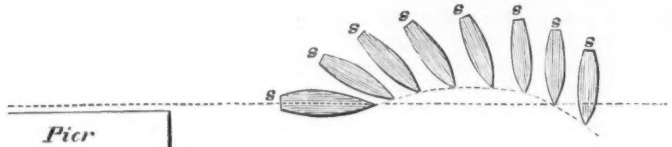
No. 1.

No. 2.

shows my idea of what actually happened, No. 2 what might have happened if the instructions had been directly disobeyed; had the vessels then struck, it would have been a glancing blow, which would not have seriously injured the steamer.

It is to be hoped that a series of experiments as to the amount of this deflection and its relation to the length, draught, and speed of the vessel might result (if it is not presumptuous in a landsman to say so) in a modification of the sailing instructions for such critical cases as I have above instanced, as may avert collisions arising from similar causes in the future.

Since a conversation on this subject here incited by the accounts of two quite recent collisions of English ironclads, a friend informs me that he has observed a steamboat leaving a wharf in Toronto Bay, which he carefully lined by means of a stationary object on the island opposite. The boat was steaming directly away from him at five or six knots, when the helm was ported, and he saw that the *whole vessel* was deflected to the left side of the line on which she had been travelling, and that it was some seconds after the helm had been ported before her bows crossed that line. Thus:—



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WATCHED BY THE DEAD.*

IN an essay on "A Novelist's Favourite Theme,"† the present writer has shown that in all Dickens's novels except "Oliver Twist" ("Pickwick" was not a novel), he presented the picture of a villain or a hypocrite, watched by one whom he despised or regarded lightly, but by whom he was eventually brought to justice. We might even have included "Oliver Twist," if it be considered how little either Sikes or Fagin suspect the watch by which Nancy defeats the projects of the craftier villain and of his dupe Monks. However, the theme is so repeatedly worked into the plots of Dickens's chief works that it was hardly necessary to note a slighter use of it such as this, except by way of showing that the whole series of stories, long and short, is thus accounted for. No one can doubt, we think, after he has examined all the evidence collected in the mentioned essay, that there was something enthralling for Dickens in this thought of a steadfast watch by an unseen or unnoted enemy, a constant danger lurking where no danger at all was suspected.

It will have been noticed further, by those who followed us through the survey of Dickens's works in search for this theme, that repeated reference was made to the use of Dickens's favourite idea in his last, but unfinished, work. There, it seems to us, the theme was to have been introduced in its most striking form, in that form which Dickens himself had mentioned in "Martin Chuzzlewit" as most terrible to think of. Dickens had pictured in his latest completed novel a man supposed to be murdered, but really alive, and watching the associates of the dead murderer of the man mistaken for him; and therein he had come very near to the idea he had pictured as the most terrible of all forms of his favourite theme. How he had enjoyed this embodiment of his theme one sees in reading the scene where the inspector proposes to arrest Harmon as an accessory in his own supposed murder. Of all the strange experiences Mr. Inspector had had, that he admits was the strangest. It had for him, Dickens tells us, all the interest of a clever conundrum, the answer to which he had been utterly unable to guess, and, "giving it up," had been told. Here Dickens had come as near as he had till then found it possible to come to the supreme horror—that *the* dead should confront his murderer—not only (as in "Our Mutual Friend") some man supposed to be murdered should confront the associates of the supposed murderer, nor even that a man supposed to be dead should confront a murderer, but that a man supposed to be murdered should keep untiring watch upon the man who supposed himself the murderer. Running through our former essay will be found the idea that this supreme horror was to have been wrought into the plot of Dickens's last novel, and that there is sufficient evidence in the work even in its incomplete state to show this.

We propose now, not to examine the story in detail in regard to the mystery of Edwin Drood's fate, which would occupy much more space than could here be spared, but to touch on certain features of the story which have hitherto been little noticed, if they have been noticed at all, and to consider certain general principles in regard to the interpretation of mysteries such as those involved in this fine unfinished novel.

First, the author's idiosyncrasies must in all such cases be considered very carefully. Some novelists like to disclose the meaning of the events described early in a story, so as to leave the reader in no manner of doubt as to the real position of affairs. Thackeray had few secrets from his readers; and accordingly the story which he left unfinished leaves no more doubt as to the ending than if it had been completed and the reader had turned to the last chapter before he had reached the end of the first volume. Dickens's method was different. He always left his readers—even the keenest—in doubt as to the actual interpretation of mysterious matters introduced early in the story, and as to the precise way in which the story was to end. But he was careful, nevertheless, to introduce a number of little details which were afterwards found to have been significant even on these points, and to have been quite clear for clear-sighted readers on some matters which the duller readers supposed to be mysterious. For instance, while I suppose no one guesses up to the last chapter of "Little Dorrit" the nature of the plot in which Rigaud-Blandois, Flintwinch, and Mrs. Clennam were concerned, or the way in which the story is to end, yet every one of any keenness knows that the old house is to fall before the story ends. Dickens not only made that clear, he *meant* to make it clear. By a curious accident, the fall of a house excited a great deal of attention a few days before the last section of "Little Dorrit" appeared, and several newspaper critics asserted that Dickens had cleverly availed himself of the interest in that catastrophe to add an effective scene to his novel. He pointed out, however, that he had been at the pains again and again to describe the premonitions of the coming fate of the old house. And every attentive reader had known that the house was doomed.

We may fairly assume, then, that in like manner the details of "The Mystery of Edwin Drood" were concealed; and the actual end of the story was not revealed except to Miss Hogarth, and in part to Mr. Forster, while, nevertheless, the broader features of the mystery were not hidden from the keener class of readers, whose enjoyment of the story Dickens well knew would be enhanced by the recognition of the general character of the plot. Dickens did, indeed, express to Miss Hogarth the fear that the Datchery assumption had been so handled in the last chapter (written) as to disclose too much; but he could hardly have intended it to disclose nothing. No one knew better than he the zest with which those who really appreciated his work would enjoy the humour and pathos of this assumption. He would not have left his best readers to obtain this pleasure (the kind of pleasure which is most noteworthy in the reading of all his novels), from a second reading, after the plot was known, as with the duller readers of his stories was commonly the case. The keenest reader would have—or rather we must unfortunately say, the keenest reader *has*—quite enough to interest him in the way of unexplained mystery; but the general interpretation of the "Datchery assumption" cannot be mistaken by any who have really studied Dickens's ways.

Albeit we may pause here for a moment to ask how far any novel, turning chiefly on a great mystery, might have been interpreted if the writer's pen had been disabled ere yet the work was completed. We have already done this with one of Dickens's own works, and we may find another example in "Our Mutual Friend." Does any one from the beginning doubt that Julius Handford, John Rokesmith, and John Harmon are one and the same person? Can any one imagine that Dickens did not mean his readers to note the confusion of Handford under the inspector's questioning, and to see even at that early stage that the body found by Gaffer Hexham is not really John Harmon's. But if that point had been overlooked, a number of others would have been decisive that the whole plot of the story was to turn on the "Rokesmith assumption," and that John Rokesmith was no other than John Harmon. Mrs. Boffin's sudden

* This article, under the title "Aftermath," forms the last chapter of a little book presently to be published by Messrs. W. H. Allen & Co., under the title "Watched by the Dead."

† *Cornhill Magazine* for March 1886.

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feeling that the child she had loved had been near her, she knew not how; Rokesmith's start when he heard the name of the adopted child (his own name); his remarks about John Harmon: these all obviously show who he is, and Dickens meant them to be so understood. We have no doubt he meant the identity of Datchery to be similarly recognised by those who knew his method. (Of course, we have very decisive evidence in regard to "Our Mutual Friend" that Dickens did not care to make any mystery of the Rokesmith assumption; for long before the end of the story we find Rokesmith talking about the details of the events which had attended his disappearance from among living men. There is no reason to suppose that he intended to be at all more careful about the identity of Datchery—a little later on though, perhaps, than the story actually reached.)

But it may be well to consider, in passing, two other stories, each turning in marked degree on a mystery—viz. "By Proxy" and "The Moonstone." The former novel admirably illustrates our subject. For although the plot is about as unlike that of "Edwin Drood" as any plot could well be, we have in "By Proxy" a man supposed to be dead, watching the man who is guilty of worse than murder—seeing that he has withheld the price at which he had, as he believes, bought his own life at the cost of a better one. Now in "By Proxy" it is essential to the interest of the latter part of the story that the reader shall not be able to conceive *how* Conway can have been saved, while yet he shall feel that Conway *must* be alive. Accordingly we have not the slightest direct suggestion of Conway's escape from death, except in the rush of a hasty messenger past the flying wretch Conway had saved. Yet we *know* that somehow Conway's escape has been achieved: and at a stage of the story corresponding to that which "The Mystery of Edwin Drood" had reached when the pen fell from the author's hand we have decisive evidence that Conway lives; nor does any one fail to understand the assumption by which he is enabled to meet his daughter without announcing his escape to the man for whom he has designed a fearful punishment.

The mystery in "The Moonstone" is altogether different in character, yet equally serves to illustrate our argument; because in "The Moonstone," as in "By Proxy," "Edwin Drood," and "Our Mutual Friend," the chief interest of the plot (we do not say of the novel as a whole, be it understood) turns on the solution of the mystery. It is noteworthy that, while in the other stories the reader is left in little doubt as to the general solution of the mystery, though altogether doubtful as to details, in "Edwin Drood" and "The Moonstone" even the general meaning of the events described in the earlier pages is left in doubt, at least for all except the very keenest readers. There are touches in the chapters of "Edwin Drood" preceding Edwin's disappearance which show any one who understands Dickens's manner, and has an ear for the music of his words, that Edwin Drood is not actually to be killed, and that the Drood who really is to be seen no more is the light-hearted whimsical boy of the earlier pages. But that evidence was not for all readers. It may even be doubted whether Dickens himself knew how clearly he had disclosed Edwin's real fate for those who knew his voice, any more than many of us know how full of meaning are certain tones of our voice for those who know us well. Nor can one analyse the effect of such tones in a written story any more than in the speaking voice. In reading "Edwin Drood," we (who write these lines) never felt any doubt from the first page to the last that Drood was to be one of the living characters at the close of the story. Yet we could not have given any definite reason for the faith that was in us, until at least the scene where Grewgims tells Jasper that Edwin and Rose had cancelled their plighted troth. In "The Moonstone" no one, we should imagine, has any idea as to the real solution, numerous though the facts are which that solution, and that alone, is to explain. There arises a vague suspicion, as we read about Frank's smoking, his sleeplessness, the difference with the doctor, and afterwards that Frank had slept well on the night of the robbery, that these little details are significant. *Possibly*, if the story had been left unfinished, the same kind of analysis which we have ourselves given to "The Mystery of Edwin Drood" might have led to these points being so carefully considered and put together as to disclose the general nature of the interpretation of the Moonstone mystery—so much, for instance, as *this*, that Frank had himself removed the diamond when under the influence of opium, that somehow Ablewhite had got hold of it, and that in some way unknown Miss Verinder was certain Frank had taken it from her cabinet. This would have explained the position of affairs at the close of the first part of the story, and one can imagine no other explanation consistent with our certainty that Frank has not wittingly had any part in abstracting the diamond, that Miss Verinder is quite incapable of the trick attributed to her by the detective (really keen though he is shown to be), and that Ablewhite has in some way got the diamond into his hands. The details of the disappearance, however, and of course the singularly effective clearing up of the mystery in this fine story (in our opinion far the strongest of all Mr. Collins's novels), could never have been guessed, no matter how close an examination any reader might have given to the earlier part of the story. Yet even these details are suggested when as yet the end of the story is far off.

(To be continued.)

COAL.

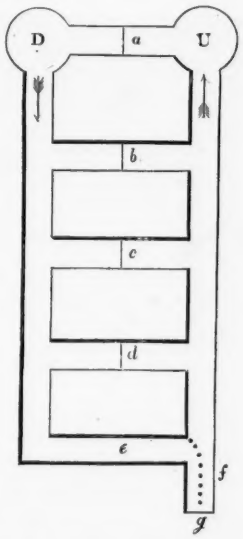
By W. Mattieu Williams.

IN my paper preceding this I described the general nature and magnitude of the problem of colliery ventilation. I will now endeavour to render intelligible the methods of ventilation that are actually adopted, omitting, for reasons already stated, any account of air-pumps, blowing machines, fans, and other devices for mechanical ventilation, and confining myself to the principle explained in my last, which in this country is almost universally applied. This principle is simply the setting of air in motion by its own gravitation by connecting two columns of air of different weights in such wise that the heavier shall descend and the lighter rise in nearly the same manner as do unequally weighted scale-pans.

As it is easier to diminish the density of a given column of air than to increase it, especially where fuel is abundant and cheap, the work of differentiation is carried out by operating on the upcast rather than the downcast shaft. We have simply to raise the mean temperature of one shaft above that of the other, and the required movement is started. The air falls in the cooler and rises in the hotter, forcing its way from the cooler to the hotter through such intervening passages as may connect them. These two shafts may be only a few yards distant from each other, and yet the current of air may be so tortured that it shall travel some miles in passing between.

The diagram shows how this is done. D represents, in horizontal section, the downcast shaft, U the upcast shaft, *a*, *b*, *c*, and *d* are passages or cross roads with doors. It is

evident that if the door at *a* is opened, the air will pass directly by the course D*a*U, as it will take the shortest course open to it. If *a* is closed and *b* open, its course will be D*b*U; if *a* and *b* are closed, and *c* is open, the course will be D*c*U; if *a*, *b*, and *c* are closed, and *d* open, the



course will be D*d*U; if all the doors are closed, the air must (neglecting the dotted line at present) take the long journey D*e*U.

In practice, of course, the arrangements are much more complex, but the principle is the same. It is easy to understand that by skilful arrangements of this kind the air may be made to take any longer or shorter course that may be desired, according to the plan of the pit roads and workings.

But supposing a thoroughfare is required, as continually happens, through a passage that must be closed to divert the ventilation—that coal has to be run through the passage *a* while the air current must pass through *e*, what must be done? This problem is solved very simply. Instead of a single door as marked in the diagram, two doors or "stoppings" are used at sufficient distance apart to allow a tub or train of tubs to stand clear between them with space to spare. The coal enters one door, this door is then shut, and it proceeds to the other.

So far I have only taken the case of continuous passages or roads, but it commonly happens that a working while in progress has no outlet at the working end—is a *cul de sac*. If the extreme distance of this from the road is but moderate and the coal is not exceptionally fiery, the current of air crossing the mouth of the working produces a stir, which, together with the natural diffusive action of gases, supplies sufficient ventilation; but when there is danger of accumulation of fire-damp in the working, a brattice is used, as shown by the dotted line at *f* in the diagram. This brattice, which is simply a partition of wood or of "brattice cloth," effects a further diversion of the current, compelling it to sweep round to the limit of the working at *g* and clear out the dangerous gases that would otherwise accumulate there until, with a limited supply of air, they formed an explosive mixture. These brattice-walls are made by simply erecting upright posts from floor to roof, and nailing the brattice-boards and cloth to them, and carefully fitting to roof and floor.

Much care, of course, is required in the working of the doors in a large pit where they are numerous and complex. A small amount of leakage from each would, of course, retard or stop the current at the extreme limits of its course.

The friction of a long journey effects considerable retardation and practically limits the distance through which the air may be forced to travel. Besides this there is another limitation. As the ventilating current proceeds it picks up the inflammable gas and the carbonic acid expired by the miners. It may even render dangerous those parts of the mine that would be otherwise safe by carrying gas from dangerous localities. Therefore in extensive collieries a system of *splits* is adopted.

This will be understood by again referring to the diagram, where, instead of all the air making the long journey D*e*U, it may be split into four independent currents, one travelling by the course D*b*U, the second by D*c*U, the third by D*d*U, and the fourth by D*e*U. But how can this be done? It is simply a matter of balancing resistances. Other conditions being equal, the resistance varies with the length of the journey; therefore, if resistances be respectively placed at *b*, *c*, and *d*, which shall be just equal to the additional resistance due to increased length of journey in getting round by *e*, the current of air will divide itself accordingly. This may be carried out by making a small opening at *b*, a somewhat larger opening at *c*, and still larger at *d*, while *e* remains fully open. In order that *all* the air shall rush through the diminished opening at *b*, it must do so at an increased velocity, and this involves increased resistance, which varies directly with the length of the journey and inversely with the sectional area of the opening. This balancing of resistance requires skilful management and the aid of regulators to vary the size of the openings as required. In some of the great collieries as much as 300,000 cubic feet of air is passed per minute through all the complications of the roads and workings.

As already stated, the power for setting all this air in motion is obtained by heating the air of the upcast shaft. Formerly a stack was built over the pit, and this was heated by a furnace on the surface. In small shafts a fire was suspended in the shaft, but these are now superseded in large workings by an underground furnace connected with the shaft by an upsloping flue which discharges all the heated products of combustion into the upcast shaft, which is thus converted into a great chimney, the contents of which may be heated to 80° or 100° above the air in the downcast shaft; 140° to 160° being temperatures commonly obtained in the upcast shaft.

The furnace used for heating is one with a large hearth and thin fire, frequently fed with small coal; the width of the fire may be 5 to 10 feet, and the fire-bars as much as 6 feet long. Thus a large area of air is heated at once.

A few figures will show the power that may thus be obtained in a deep mine such as that of Ashton Moss, which has a depth of 2,850 feet. Let us suppose the diameter of the shafts to be 12 feet each; their sectional area will be about 113 square feet, and thus each will contain 113 cubic feet of air for every foot of depth. A cubic foot of air at 60° weighs 1·29 oz. avoirdupois; ∴ 113 cubic feet weigh 9·2 lbs., and the total column of air at 60° = 2,850 × 9·2, which, in round numbers, amounts to 26,000 lbs. But in heating air from 60° to 150°, it expands one-sixth of its bulk; therefore the hot air in the upcast shaft, in such a case, will weigh one-sixth less than the cool in the downcast. This difference amounts to 4,333 lbs. Thus the *vis a tergo*, or force driving the air through the roads and workings and up the upcast shaft, will, in this case, exceed a pressure of 4,000 lbs. It would

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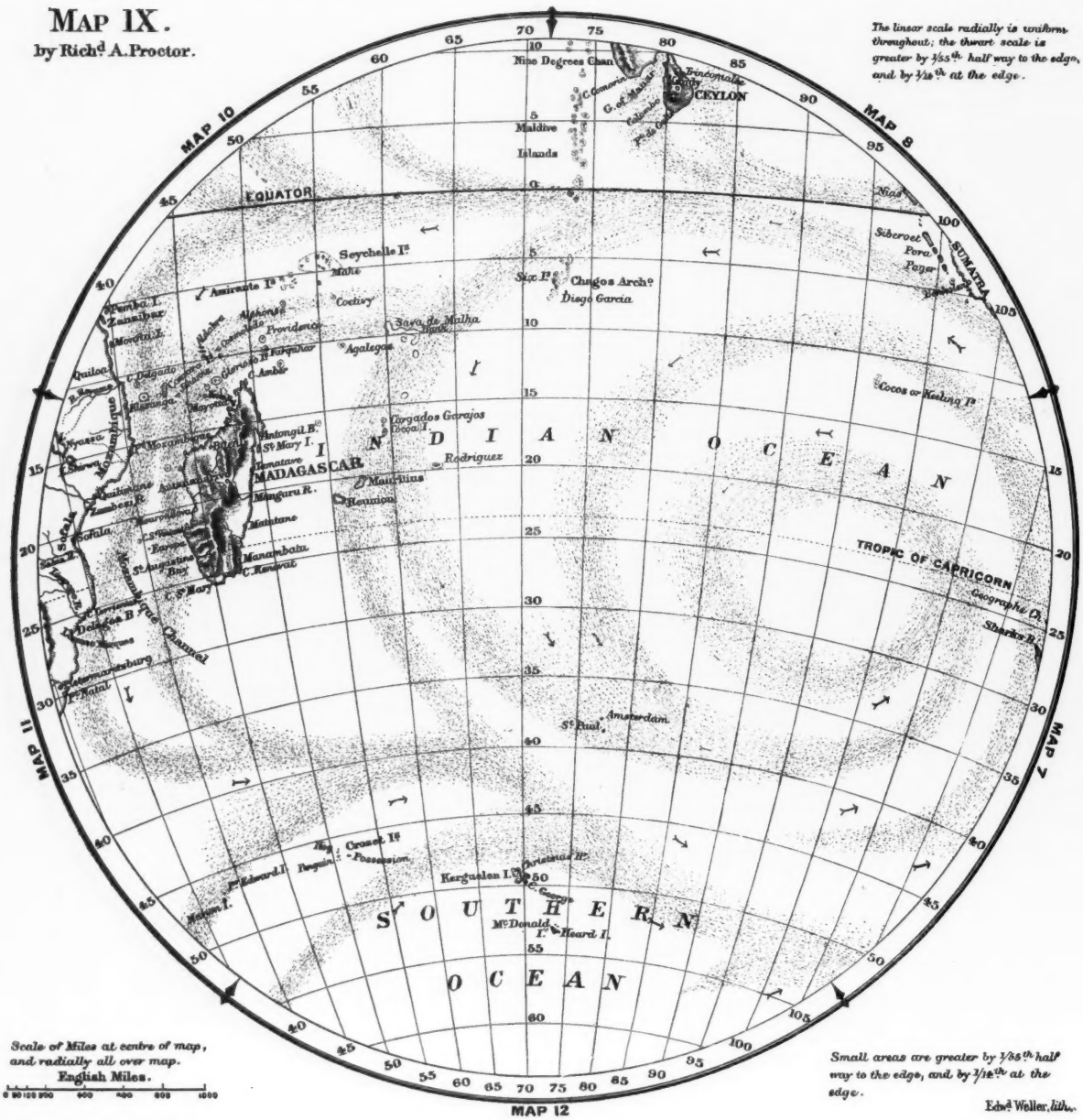
exert that amount of pressure against any unyielding resistance.

From this it will be understood how a gale of wind may be made to blow along the dark subterranean roads of a deep coal-pit. I may add that the figures above rather under-estimate the actual force, as I have taken the air at surface density. It actually increases as we descend—increasing, of course, all the quantities named.

THE ONE-SCALE ATLAS.

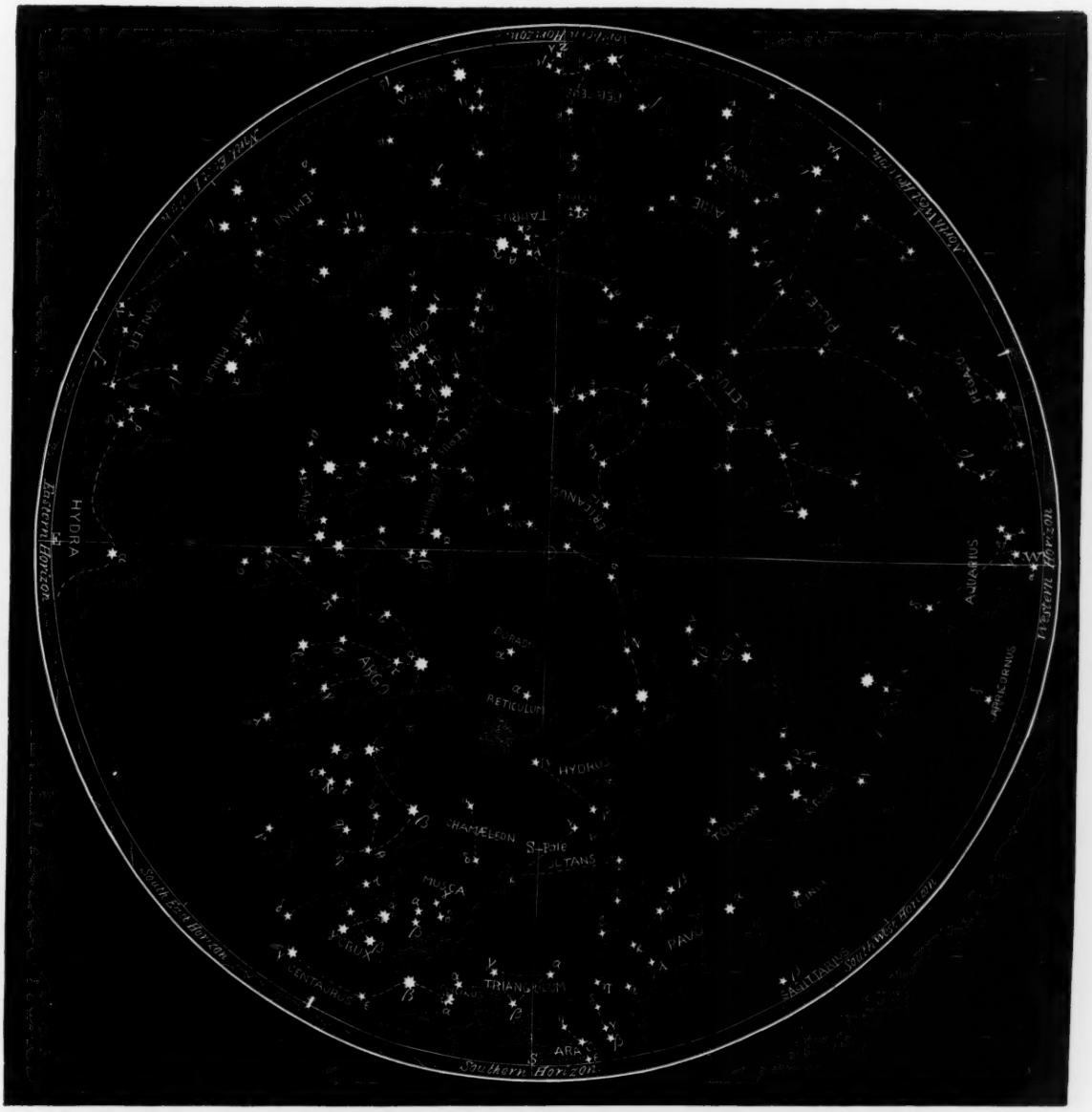
MAP IX.

by Rich^{d} A. Proctor.



THE SOUTHERN SKIES.

MAP XIII.—FOR OCTOBER, NOVEMBER, AND DECEMBER.



THE NIGHT SKIES IN THE SOUTHERN HEMISPHERE (LAT. 46° TO 24° S.)
AND THE
SOUTHERN SKIES IN ENGLAND (UPPER HALF OF MAP ONLY) AT THE FOLLOWING TIMES:

At 1 o'clock, morning, Nov. 7.	At 11 o'clock, night, Dec. 7.	At 9 o'clock, night, Jan. 7.
„ 12.30 „ „ Nov. 14.	„ 10.30 „ „ Dec. 15.	„ 8.30 „ „ Jan. 14.
„ Midnight, Nov. 22.	„ 10 „ „ Dec. 23.	„ 8 „ „ Jan. 22.
„ 11.30 o'clock, night, Nov. 30.	„ 9.30 „ „ Dec. 30.	„ 7.30 „ „ Jan. 30.

STAR MAGNITUDES.

First . . . . ✱ Second . . . . ✱ Third . . . . ★ Fourth . . . . + Fifth . . . . ▲

SELF-CHARTED STARS.

IN considering such a chart of stars as appeared in the number of KNOWLEDGE for May 1886, considerations of singular, one might almost say quaint, significance are suggested to the mind.

Among all those discs representing stars, and each telling the same wonderful and complex story, no two represent the same time. The story told by that star-chart is not simultaneous. It bears the same relation to a record presenting matters as they actually are at a given moment that a story told by a newspaper bears to a chapter in a volume of history. When we are looking at one star-disc, we contemplate the self-told record of a sun as that sun was many years ago; when we turn to another star-disc, we see a similar record, also relating to a long past epoch, but not to the same epoch. One of the star-discs, regarding it as in a sense a portrait of a remote sun, may be hundreds of years older than another, though both were taken at the same instant on the photographic plate. Probably within the range of this one picture, with its two thousand stars, we have records extending over many hundreds of years. It is true that the picture represents a rich region of the Milky Way, and my own theory of the Milky Way, adopted and supported by such independent thinkers as Herbert Spencer * in England, and Oscar Peschule in Germany, recognises the whole rich galactic mass like that seen in Cygnus as in reality what it appears to be (that is, as truly a stellar cloud, whose farthest portions are not relatively much farther from us than the nearest). Yet it can hardly be but that in the same field of view are seen many stars which do not belong to that cloud, but shine from beyond depths far more remote. Even as regards the great star-cloud itself, moreover, we have a wide range of time to deal with. Judging from the length and width of the great clustering aggregation of the Milky Way in Cygnus, from the heart of which the chart has been taken, we may infer a depth corresponding to more than one-tenth of the distance of the star-cloud regarded as a whole. Supposing this distance, then, to correspond to a light-journey of one hundred years—a very moderate assumption when we remember that Sirius lies at a distance of about twenty years' light-journey from us—we find that the farthest stars of the cluster tell us their story ten years after the nearest stars.

Thus the strange thought arises that while the chart does not represent the actual state of the pictured stars at the moment when the photograph was taken, it does not even represent the state of those stars—either as to relative position or relative lustre—at any epoch whatsoever. For aught we can tell any star taken at random in the chart may have been extinct many years before it painted its record on the photographic plate; or it may at the moment when its light reached us have been in reality very much brighter or very much fainter than its pictured record would seem to show. Stars may in the meantime have shone forth which are not shown in the chart, because the extremities of the light-pencils by which alone their discs could be delineated had not reached within many thousands of millions of miles of our solar system. Then, again, not a single star in the whole chart is shown in its true position, and no two stars are shown in their proper relative position. For every star of the two thousand travelling along through space must have some thwart motion, greater or less (we may exclude as impossible the case of a star moving *exactly* towards the solar system); and no two stars can have exactly the same thwart motion.

Hence have we this strange result, that a photographic record which the astronomer justly regards as a most marvellously exact star-chart, far more exact than any such record as the most skilful astronomers can obtain observationally, nevertheless neither represents what is now, or what ever was—precisely as here shown—at any moment of time.

* As regards important portions of my theory of the universe I was anticipated by Mr. Herbert Spencer—though I only learned this long after I had advanced that theory. But I am speaking above of the Milky Way regarded as a congeries of stars, in which aspect I believe I may say that I alone established those details in which my theory differs from that of the Herschels formerly accepted.

MAGIC SQUARES.*

BY W. HOLDEN.

AS I have not been hitherto "A Constant Reader of your valuable Periodical," I do not know if you have dealt with the subject of "Magic Squares" or not; but I have thought that the enclosed would be interesting to your readers. They appear to me to be marvels of ingenuity. About fifteen years ago the methods of constructing these "Magic Squares," so called, was discussed in "The Riddler" columns of the *Adelaide Observer*, of which columns I have the editing. It appears that the rules for constructing squares, of which the root is an odd number as 7×7, is very simple. Thus I have in my possession a square 27×27, containing all the numbers from 1 to 27^2, and of course the sum of every horizontal, vertical, and diagonal line is $27 \times \frac{27^2 + 1}{2} = 9{,}855$. But the method of constructing it is so simple that the figures may be written down consecutively without the slightest hesitation. The case, however, is very different when the root is an even number, and especially so when it is the double of a prime number as 14×14. But at the time to which I refer, Mr. E. J. Catlow, who was then engaged as a shepherd in the interior of this "island continent," turned his attention to the subject, and discovered the rules for constructing squares of every kind, no matter how large. He subsequently told me that if he had not turned his attention to the subject, and to the German language, his occupation was so monotonous, and the solitude so profound, that he feared he would have gone insane.

Some time after his discovery of the rules referred to, Mr. Catlow wrote a paper describing them, and sent it to Mr. C. Todd, C.M.G., our Postmaster-General and Government Astronomer. The paper was read by Mr. Todd before the Adelaide Philosophical Society, and he stated that in all his mathematical studies he had never met with any rules of the kind applicable to all numbers.

Curiously enough, however, while Mr. Catlow was studying the subject in the far north, Mr. J. B. Bassett, a schoolmaster of Willunga, thirty miles south of Adelaide, was doing the same thing, and the result was the discussion in the *Adelaide Observer* as to priority of discovery, to which I have referred.

Mr. Catlow and Mr. Bassett have both "joined the great majority," but among the papers of the latter was the square of which No. 1 is a copy. You will observe

* Although in the earlier volumes of KNOWLEDGE we had a great number (some said too large a number) of magic squares, and have in MS. solutions of the still more complex puzzles presented by magic cubes, we print Mr. Holden's paper unhesitatingly, as presenting novel and interesting developments.

that all the numbers from 1 to 32, and from 225 to 256—*i.e.*, the sixteen lowest and the sixteen highest—fill the two top and the two bottom horizontal lines, and that the order

No. 1. COMPOUND MAGIC SQUARE.
Constructed by the late Mr. J. B. Bassett, by rules of his own invention. October 19, 1869.

2	239	242	31	4	237	244	29	6	235	246	27	8	233	248	25
241	32	1	240	243	30	3	238	245	28	5	236	247	26	7	234
34	207	100	184	169	89	72	148	141	150	139	123	102	125	210	63
209	64	104	36	205	212	61	153	122	38	203	214	59	135	33	208
47	194	121	211	62	35	206	136	103	213	60	37	204	154	223	50
224	49	137	45	196	221	52	120	138	43	198	219	54	119	48	193
66	175	152	222	51	46	195	105	151	220	53	44	197	106	178	95
177	96	109	73	88	168	185	157	132	107	118	134	155	116	65	176
79	162	147	170	183	71	90	99	126	149	140	101	124	142	191	82
192	81	70	40	201	216	57	187	165	68	173	180	93	92	80	161
98	143	188	215	58	39	202	69	91	179	94	67	174	166	146	127
145	128	171	41	200	217	56	86	182	77	164	189	84	75	97	144
111	130	85	218	55	42	199	172	76	190	83	78	163	181	159	114
160	113	158	87	74	186	167	110	115	108	117	156	133	131	112	129
15	226	255	18	13	228	253	20	11	230	251	22	9	232	249	24
256	17	16	225	254	19	14	227	252	21	12	229	250	23	10	231

Sum of each vertical, horizontal, and diagonal column of the largest square = 2,056; of the inner = 1,542; and of each of the smallest = 514. Hence the relative value of the sum of each column of the several squares is as 4 . 3 . 1.

of their succession is similar to the knights' leaps over the chess-board. So far the rule is fairly simple, but the arrangement of all the intermediate numbers, so far as I have examined the table, seems quite capricious. Yet it must have been done upon some recognised system, or it could not have been done at all.

I have failed in my attempts to discover the rules for the construction of No. 2. Mr. Campion is a storekeeper. He was in Adelaide about a month ago, and he informed me that he discovered at the beginning of the present year the rules for constructing all such squares. In proof of this, he has since then sent me a "Jubilee Magic Square," 50 × 50. It covers a sheet of paper about four feet square, and is perfect in every respect. Thus the sum of each column of the outer square is 62,525, with a constant subtrahend of 2,501 on approaching the centre. The subjoined was published in "The Riddler" a few weeks ago :—

No. 2. COMPOUND MAGIC SQUARE.
Constructed by Mr. W. F. Campion, by rules of his own invention 1887.

242	238	234	230	1	5	9	13	18	22	26	30	253	249	245	241
3	213	208	204	32	35	39	43	223	48	52	56	219	215	212	254
6	31	190	186	182	57	61	65	70	74	78	197	193	189	226	251
10	36	59	169	164	80	83	87	175	92	96	171	168	198	221	247
14	40	62	79	154	150	97	101	106	110	157	153	178	195	217	243
21	47	66	84	99	141	112	115	143	120	140	158	173	191	210	236
25	51	73	91	102	111	136	125	131	122	146	155	166	184	206	232
29	55	77	95	109	119	124	127	129	134	138	148	162	180	202	228
255	224	199	176	159	144	121	130	128	135	113	98	81	58	33	2
250	220	194	172	152	139	133	132	126	123	118	105	85	63	37	7
246	216	188	167	149	117	145	142	114	137	116	108	90	69	41	11
240	211	185	163	104	107	160	156	151	147	100	103	94	72	46	17
237	207	181	89	93	177	174	170	82	165	161	86	88	76	50	20
233	203	68	71	75	200	196	192	187	183	179	60	64	67	54	24
229	45	49	53	225	222	218	214	34	209	205	201	38	42	44	28
16	19	23	27	256	252	248	244	239	235	231	227	4	8	12	15

Sum of each vertical, horizontal, and diagonal column of the outer square, 2,056. By the successive removal of the margins the sum of each horizontal, vertical, and diagonal column is reduced by 257. Hence the sums are respectively 1,799, 1,542, 1,285, 1,028, 771, and 514.

Mr. W. F. Campion has furnished us with several more curious magic squares, including the following :—

CHINESE MAGIC SQUARE, 9 × 9.

77	4	3	2	1	72	71	70	69
14	62	19	18	17	58	57	56	68
15	27	51	30	29	48	47	55	67
16	28	36	44	37	42	46	54	66
9	23	33	39	41	43	49	59	73
74	60	50	40	45	38	32	22	8
75	61	35	52	53	34	31	21	7
76	26	63	64	65	24	25	20	6
13	78	79	80	81	10	11	12	5

In this instance the sum of each vertical, horizontal, and diagonal column of the interior square of 3 × 3 is 123; that of the next, namely, 5 × 5, is 205; that of the next, namely, 7 × 7, 287; and that of the outermost 369; thus the sum of every column is a multiple of 41.

NOTE ON EUCLID (I. 32).

(PROP. 3, BK. I.; AXIOM 12, BK. I.; AND PARALLELS).

A CORRESPONDENT asks whether Euclid I. 32 cannot be proved by mechanical considerations independently of axiom 12.

It has always seemed to me that the proposition is self-evident, the mind at once picturing some such angular measurement as is shown in fig. 1. The arrowed rod PQ, pivoting round A to position pq, measures the angle A; then, pivoting round B to position $p'q'$, measures the angle B;

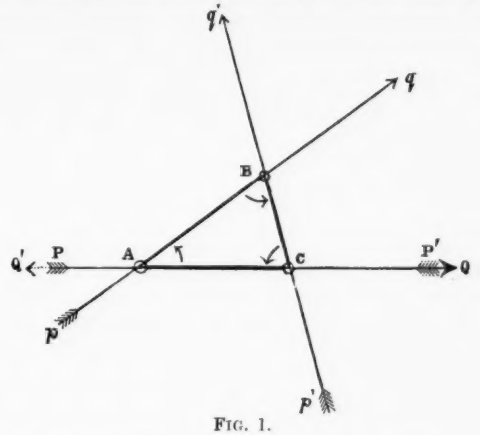


FIG. 1.

and lastly, pivoting round C to position P′Q′, measures the angle C. It has manifestly pivoted through two right

angles, lying now in the same direction as at first, with changed ends. Hence the three angles, which are together equal to the angle through which the rod has pivoted in all, amount together to two right angles.

With regard to axiom 12, and the treatment of the whole subject of parallelism and non-parallelism in Euclid, the trouble seems to arise from two causes: First, the use of a negative definition of parallel lines, instead of a positive definition from which the negative property could be deduced; and secondly, from the wish to prove certain matters really axiomatic—for instance, that opposite or vertical angles are equal. The axiomatic ideas underlying axiom 12, which as presented in Euclid (with its converse, Euc. I. 16, as a *proposition* and in company with *propositions* relating to parallels) is not axiomatic at all, may be thus dealt with:—

If the straight lines AB, DE, crossing in C, be supposed shifted so that the point C falls at G in AB, AB remaining unchanged in position, and DE falling into the position FGH, FH can nowhere cut DE. For any argument showing that GF produced towards F would cut CD produced towards D,

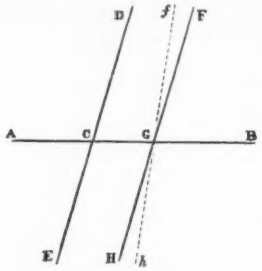


FIG. 2.

would equally show that CE produced towards E would cut GH produced towards H. And if both intersections occurred, the two straight lines DE and FH produced far enough would enclose a space.

This, however, is not axiomatic, unless we take it for granted that the angle FGB, which (we see from the way in which it was obtained) is the same as the angle DCB, is equal to CGH, which (for a like reason) is the same as the angle ACE; for the obviousness of the argument depends on the fact that we have the same arrangement precisely when we look at the figure as it stands that we have when we reverse it.

Now it is not one whit more obvious that two straight lines will not enclose a space than it is that opposite angles are equal, or that two straight lines *can* in any way be so drawn as never to meet.

After it is seen that FGH obtained by moving ACD to position AGF will not cut DCE, it ought to be equally obvious that any straight line G*f* through G on the side of GF towards D *must* cut CD, no matter how small the angle *f*GF may be. For the point *f* is *some* distance from GF, and by doubling G*f* this distance will be doubled (this may be proved by superposition), and the doubled distance will in turn be doubled in the same way (by quadrupling G*f*), and so on as long as we please: hence we must at length by successive doublings obtain a distance less than the distance of any point in ED from FH; in other words, G*f* produced far enough must pass to the side of EF remote from FH—that is, G*f* will eventually cut ED.

Or we may put the matter in the form of a general axiom,—Through a given point (G), outside a given straight line (DCE), only one parallel can be drawn to that line.

These properties, whether presented as axioms or reasoned out, serve all purposes. But in reality, if such matters are to be reasoned out, then the statement that two straight lines cannot enclose a space should be reasoned out too; and I do not envy any one who makes the attempt. In fact, it leads directly to the discussion of non-Euclidean geometry, a part—and a useless part—of Dream Mathematics.

Of the following three propositions, *which*, dealt with as a subject for reasoning, would be the more difficult?—

First, *Through a given point there cannot be drawn a straight line in the same plane as another straight line, which, being produced sufficiently far both ways, shall meet the latter straight line on both sides of the given point.*

Secondly, *Through a given point there can be drawn a straight line in the same plane as another straight line, which, however far it be produced either way, shall not meet the latter straight line on either side of the given point.*

Thirdly, *Through a given point there can only be drawn one straight line in the same plane as another straight line, which, however far it be produced either way, shall not meet the latter straight line on either side of the given point.*

A FIVE-FOLD COMET.

THE figure illustrating this article is taken from *L'Astronomie*, and represents the remarkable Southern Comet of January last, as drawn on successive days by Mr. Finlay, of Cape Town.

The comet was first seen by a farmer and a fisherman of Blauwberg, near Cape Town, on the night of January 18–19. The same night it was seen at the Cordoba Observatory by M. Thome. On the next night Mr. Todd discovered it independently at the Adelaide Observatory, and watched it till the 27th. On the 22nd Mr. Finlay detected the comet, and was able to watch it till the 29th. At Rio de Janeiro M. Cruls observed it from the 23rd to the 25th; and at Windsor, New South Wales, Mr. Tebbutt observed the comet on the 28th and 30th. Moonlight interfered with further observations.

The comet's appearance was remarkable. Its tail, long and straight, extended over an arc of 30 degrees, but there was no appreciable condensation which could be called the comet's head. The long train of light, described as nearly equal in brightness to the Magellanic clouds, seemed to be simply cut off at that end where in most comets a nucleus and coma are shown.

This comet has helped to throw light on one of the most perplexing of all the puzzles which those most perplexing of all the heavenly bodies, comets, have presented to astronomers.

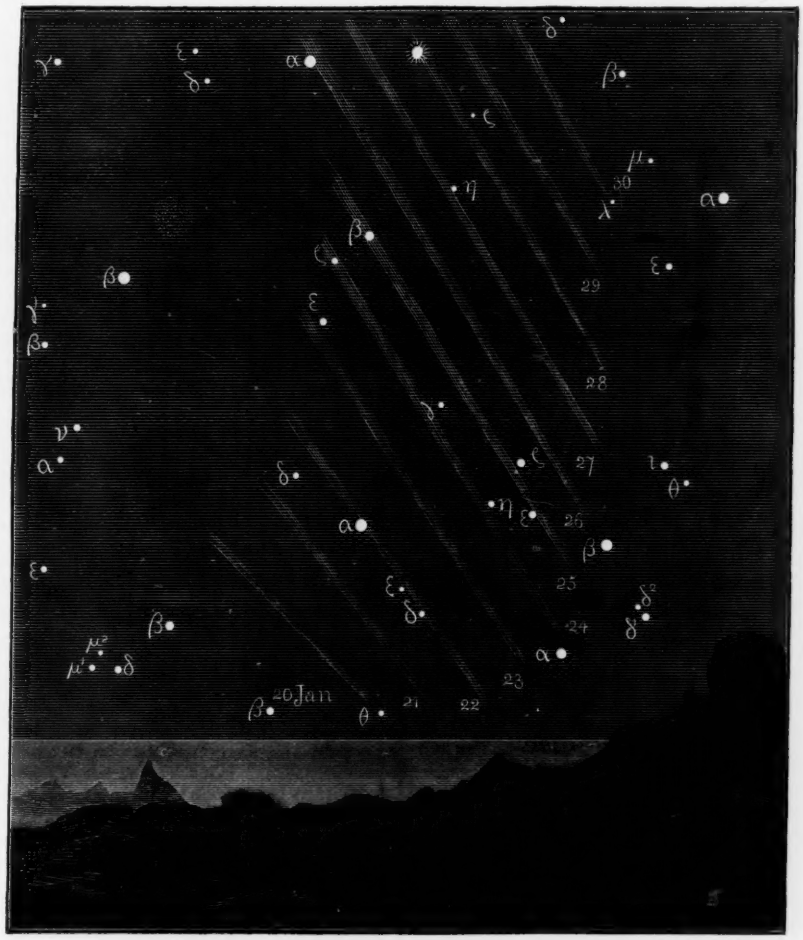
In the year 1668 a comet was seen in the southern skies which attracted very little notice at the time, and would probably have been little thought of since had not attention been directed to it by the appearance and behaviour of certain comets seen during the last half-century. Visible for about three weeks, and discovered after it had already passed the point of its nearest approach to the sun, the comet of 1668 was not observed so satisfactorily that its orbit could be precisely determined. In fact, two entirely different orbits would satisfy the observations fairly, though one only could be regarded as satisfying them well.

This orbit, however, was so remarkable that astronomers were led to prefer the other, less satisfactory though it was in explaining the observed motions of the comet. For the orbit which best explained the comet's movements carried the comet so close to the sun as actually to graze his visible surface.

Moreover there was this remarkable, and, indeed, absolutely unique peculiarity about the orbit thus assigned: the comet (whose period of revolution was to be measured by hundreds of years) actually passed through the whole of that part of its course during which it was north of our earth's orbit-plane in less than two hours and a half! though this part of its course is a half-circuit around the sun, so far as direction (not distance of travel) is concerned. That comet, when at its nearest to the sun, was travelling at the rate of about 330 miles per second. It passed through regions near the sun's surface commonly supposed to be occupied by atmospheric matter.

calculate the return of comets.) The comet of 1680, called Newton's, was the very first whose orbital motions were dealt with on the principles of Newtonian astronomy, and Halley's was the first whose periodic character was recognised.

In 1843 another comet came up from the south, and presently returned thither. It was, indeed, only seen during its return, having, like the comet of 1668, been only discovered a day or two after perihelion passage. Astronomers soon began to notice a curious resemblance between the orbits of the two comets. Remembering the comparative roughness of the observations made in 1668, it may be



The Constellations, though unnamed, can readily be identified, when it is noted that the Comet's course, as here presented, began in the constellation of the Crane.

Now, had the comet been so far checked in its swift rush through those regions as to lose one-thousandth part of its velocity, it would have returned in less than a year. But the way in which the comet retreated showed that nothing of this sort was to be expected. I am not aware, indeed, that any anticipations were ever suggested in regard to the return of the comet of 1668 to our neighbourhood. It was not till the time of Halley's comet, 1682, that modern astronomy began to consider the question of the possibly periodic character of cometic motions with attention. (For my own part, I reject as altogether improbable the statement of Seneca that the ancient Chaldean astronomers could

said that the two comets moved in the same orbit, so far as could be judged from observation. The comet of 1843 came along a path inclined at apparently the same angle to the earth's orbit-plane, crossed that plane ascendingly at appreciably the same point, swept round in about two hours and a half that part of its angular circuit which lay north of the earth's orbit-plane, and crossing that plane descendingly at the same point as the comet of 1668, passed along appreciably the same course towards the southern stellar regions! The close resemblance of two paths, each so strikingly remarkable in itself, could not well be regarded as a mere accidental coincidence.

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However, at that time no very special attention was directed to the resemblance between the paths of the comets of 1843 and 1668. It was not regarded as anything very new or striking that a comet should return after making a wide excursion round the sun; and those who noticed that the two comets really had traversed appreciably the same path around the immediate neighbourhood of the sun, simply concluded that the comet of 1668 had come back in 1843, after 175 years, and not necessarily for the first time.

It must be noticed, however, before leaving this part of the record, that the comet of 1843 was suspected of behaving in a rather strange way when near the sun. For the first observation, made rather roughly, indeed, with a sextant, by a man who had no idea of the interest his observation might afterwards have, could not be reconciled by mathematicians (including the well-known mathematician, Benjamin Pierce) with the movement of the comet as subsequently observed. It seemed as though when in the sun's neighbourhood the comet had undergone some disturbance, possibly internal, which had in slight degree affected its subsequent career.

According to some calculations the comet of 1843 seemed to have a period of about thirty-five years, which accorded well with the idea that it was the comet of 1668, returned after five circuits. Nor was it deemed at all surprising that the comet, conspicuous though it is, had not been detected in 1713, 1748, 1783, and 1818, for its path would carry it where it would be very apt to escape notice except in the southern hemisphere, and even there it might quite readily be missed. The appearance of the comet of 1668 corresponded well with that of the comet of 1843. Each was remarkable for its extremely long tail, and for the comparative insignificance of its head. In the northern skies, indeed, the comet of 1843 showed a very straight tail, and it is usually depicted in that way, whereas the comet of 1668 had a tail showing curvature. But pictures of the comet of 1843, as seen in the southern hemisphere, show it with a curved tail, and also the tail appeared forked towards the end during that part of the comet's career.

However, the best observations, and the calculations based on them, seemed to show that the period of the comet of 1843 could not be less than 500 years.

Astronomers were rather startled, therefore, when, in 1880, a comet appeared in the southern skies, which traversed appreciably the same course as the comets of 1668 and 1843. When I was in Australia, in 1880, a few months after the great comet had passed out of view, I met several persons who had seen both the comet of that year and the comet of 1843. They all agreed in saying that the resemblance between the two comets was very close. Like the comet of 1843, that of 1880 had a singularly long tail, and both comets were remarkable for the smallness and dimness of their heads. One observer told me that at times the head of the comet of 1880 could barely be discerned.

Like the comets of 1665 and 1843, the comet of 1880 grazed close past the sun's surface. Like them it was but about two hours and a half north of the earth's orbit-plane. Had it only resembled the other two in these remarkable characteristics, the coincidence would have been remarkable. But of course the real evidence by which the association between the comets was shown was of a more decisive kind. It was not in general character only but in details that the path of the comet of 1880 resembled those on which the other two comets had travelled. Its path had almost exactly the same slant to the earth's orbit-plane as theirs, crossed that plane ascendingly and descendingly at almost exactly the same points, and made its nearest approach to the sun at very nearly the same place. To the astronomer such evidence is decisive. Mr. Hind, the Superintendent of the "Nautical Almanac," and as sound and cautious a student of cometic astronomy as any man living, remarked so soon as the resemblance of these comets' paths had been ascertained, that if it were merely accidental the case was most unusual; nay, it might be described as unique. And, be it noticed, he was referring only to the resemblance between the comets of 1880 and 1843. Had he recalled at the time the comet of 1668, and its closely similar orbit, he would have admitted that the double coincidence could not possibly be merely casual.

But this was by no means the end of the matter. Indeed, thus far, although the circumstances were striking, there was nothing to prevent astronomers from interpreting them as other cases of coincident, or nearly coincident, cometic paths had been interpreted. Hind and others, myself included, inferred that the comets of 1880, 1843, and 1668 were simply one and the same comet, whose return in 1880 probably followed the return in 1843 after a single revolution.

In 1882, however, two years and a half after the appearance of the comet of 1880, another comet came up from the south, which followed in the sun's neighbourhood almost the same course as the comets of 1668, 1843, and 1880. The path it followed was not quite so close to those followed by the other three as these had been to each other, but yet was far too close to indicate possibly a mere casual resemblance; on the contrary, the resemblance in regard to shape, slope, and those peculiarities which render this family of comets unique in the cometary system, was of the closest and most striking kind.

Many will remember the startling ideas which were suggested by Professor Piazzi Smyth respecting the portentous significance of the comet of 1882. He regarded it as confirming the great pyramid's teaching (according to the views of orthodox pyramidalists) respecting the approaching end of the Christian dispensation. It was seen under very remarkable circumstances, blazing close by the sun, within a fortnight or three weeks of the precise date which had been announced as marking that critical epoch in the history of the earth.

Moreover, even viewing the matter from a scientific standpoint, Professor Smyth (who, outside his pyramidal paradoxes, is an astronomer of well-deserved repute) could recognise sufficient reason for regarding the comet as portentous.

Many others, indeed, both in America and in Europe, shared his opinion in this respect. A very slight retardation of the course of the comet of 1880, during its passage close by the surface of the sun, would have sufficed to alter its period of revolution from the thirty-seven years assigned on the supposition of its identity with the comet of 1843, to the two and a half years indicated by its apparent return in 1882, and if this had occurred in 1880, a similar interruption in 1882 would have caused its return in less than two and a half years.

Thus, circling in an ever narrowing (or rather shortening) orbit, it would presently, within a quarter of a century or so perhaps, have become so far entangled among the atmospheric matter around the sun, that it would have been unable to resist absolute absorption. What the consequences to the solar system might have been none ventured to suggest. Newton had expressed his belief that the effects of such absorption would be disastrous, but the physicists of the nineteenth century, better acquainted with the laws associating heat and motion, were not so despondent. Only Professor Smyth seems to have felt assured (not being despondent but confident) that the comet portended, in a very decisive way, the beginning of the end.

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However, we were all mistaken. The comet of 1882 retreated on such a course, and with such variation of velocity as to show that its real period must be measured not by months, as had been supposed, nor even by years, but by centuries. Probably it will not return till 600 or 700 years have passed. Had this not been proved, we might have been not a little perplexed by the return of apparently the same comet in this present year. A comet was discovered in the south early in January, whose course, dealt with by Professor Kruger, one of the most zealous of our comet calculators, is found to be partially identical with that of the four remarkable comets we have been considering. Astronomers have not been moved by this new visitant on the well-worn track, as we were by the arrival of the comet of 1882, or as we should have been if either the comet of 1882 had never been seen, or its path had not been shown to be so wide ranging. Whatever the comet of the present year may be it was not the comet of 1882 returned. No one even supposes that it was the comet of 1880, or 1843, or 1668. Nevertheless, rightly apprehended, the appearance of a comet travelling on appreciably the same track as those four other comets is of extreme interest, and indeed practically decisive as to the interpretation we must place on these repeated coincidences.

Observe, we are absolutely certain that the five comets are associated together in some way; but we are as absolutely certain that they are not one and the same comet which had travelled along the same track and returned after a certain number of circuits. We need not trouble ourselves with the question whether two or more of the comets may not have been in reality one and the same body at different returns. It suffices that they all five were not one; since we deduce precisely the same conclusion whether we regard the five as in reality but four or three or two. But it may be mentioned in passing as appearing altogether more probable, when all the evidence is considered, that there were no fewer than five distinct comets, all travelling on what was practically the self-same track when in the neighbourhood of the sun.

There can be but one interpretation of this remarkable fact—a fact really proved, be it noticed (as I and others have maintained since the retreat of the comet of 1882), independently of the evidence supplied by the great southern comet of the present year. These comets must all originally have been one comet, though now they are distinct bodies. For there is no reasonable way (indeed, no possible way) of imagining the separate formation of two or more comets at different times, which should thereafter travel in the same path.

No theory of the origin of comets ever suggested, none even which can be imagined, could account for such a peculiarity. Whereas, on the other hand, we have direct evidence showing how a comet, originally single, may be transformed into two or more comets travelling on the same, or nearly the same, track.

The comet called Biela's, which had circuited as a single comet up to the year 1846 (during a period of unknown duration in the past—probably during millions of years), divided them into two, and has since broken up into so many parts that each cometic fragment is separately undiscernible. The two comets into which Biela's divided, in 1846, were watched long enough to show that had their separate existence continued (visibly) they would have been found, in the fulness of time, travelling at distances very far apart, though on nearly the same orbit. The distance between them, which in 1846 had increased only to about a quarter of a million of miles, had in 1852 increased to five times that space.

Probably a few thousands of years would have sufficed to set these comets so far apart (owing to some slight difference of velocity, initiated at the moment of their separation) that when one would have been at its nearest to the sun the other would have been at its farthest from him. If we could now discern the separate fragments of the comet, we should doubtless recognise a process in progress by which, in the course of many centuries, the separate cometic bodies will be disseminated all round the common orbit. We know, further, that already such a process has been at work on portions removed from the comet many centuries ago, for as our earth passes through the track of this comet she encounters millions of meteoric bodies which are travelling in the comet's orbit, and once formed part of the substance of a comet doubtless much more distinguished in appearance than Biela's.

There can be little doubt that this is the true explanation of the origin of that family of comets, five of whose members returned to the neighbourhood of the sun (possibly their parent) in the years 1668, 1843, 1880, 1882, and 1887.*

But it is not merely as thus explaining what had been a most perplexing problem that I have dealt with the evidence supplied by the practical identity of these five comets' orbits. When once we recognise that this, and this only, can be the explanation of the associated group of five comets, we perceive that very interesting and important light has been thrown on the subject of comets generally. To begin with: what an amazing comet that must have been from which these five, and we know not how many more, were formed by disaggregative processes—probably by the divellent action of repulsive forces exerted by the sun! Those who remember the comets of 1843 and 1882 as they appeared when at their full splendour will be able to imagine how noble an appearance a comet would present which was formed of these combined together in one. But the comet of 1880 was described by all who saw it in the southern hemisphere as most remarkable in appearance, despite the faintness of its head. The great southern comet of the present year was a striking object in the skies, though it showed the same weakness about the head. That of 1668 was probably as remarkable in appearance as even the comet of 1882. A comet formed by combining all these together would certainly surpass in magnificence all the comets ever observed by astronomers.

And then, what enormous periods of time must have been required to distribute the fragments of a single comet so widely that one would be found returning to its perihelion more than two centuries after another! When I spoke of one member of the Biela group being in aphelion when another would be in perihelion, I was speaking of a difference of only three and one-third years in time; and even that would require thousands of years. But the scattered cometic bodies which returned to the sun's neighbourhood in 1668 and 1887 speak probably of millions of years which have passed since first this comet was formed. It would be a matter of curious inquiry to determine what may have been the condition of our sun, what even his volume, at that remote epoch in history.

* It may be interesting to compare the orbital elements of the five comets above dealt with. They may be presented as follows; but it should be noticed that the determinations must be regarded as rough in the case of Comets I. and V., as the observations were insufficient for exact determination of the elements:—

	I.	II.	III.	IV.	V.
Perihelion Passage	1668, Feb. 29	1843, Feb. 27	1880, Jan. 27	1882, Sep. 17	1887, Jan. 11
Log. Per. Dist.	7·6721	7·8395	7·7714	7·8895	8·1644
Long. Per.	80° 15′	73° 30′ 46″	74° 11′ 13″	55° 37′ 29″	89° 41′
Long. Node	357° 17′	355° 46′ 48″	356° 17′ 4″	346° 1′ 27″	359° 41′
Inclination	125° 58′	143° 1′ 31″	143° 7′ 31″	141° 59′ 40″	141° 16′
Eccentricity	0·9999	0·9991	0·9995	0·9999	....
Calculator	Henderson	Plantamour	Meyer	Kreutz	Finlay

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SIR HENRY ROSCOE ON ATOMS.

THE general press has done scant justice to Sir Henry Roscoe's Presidential Address to the British Association, the *Times* leading the way in speaking of it as too technical both in subject and treatment. Doubtless it is so for the majority of readers, who need everything pounded into pemmican or condensed into a sort of Liebig's Extract, but not for those who desire to know as far as can be known everything about something rather than something about everything. Sir Henry wisely talked on the subject of which he knows most, and his words were as admirable and clear as his substance was weighty and suggestive.

After justifying the researches of the chemist by their economical results, he pointed out how at the back of all speculations concerning the nature of the motions of which matter is the vehicle, there lies the profoundly interesting question as to the nature and mutual relations of the atoms themselves, the building materials of the universe, and which have known no change amidst the ever-changing combinations into which they enter.

Eighty years ago, Dalton, working in no luxurious laboratory, but with the meagre apparatus of a few cups, penny ink-bottles and self-made thermometers and barometers, discovered that atoms combine in definite proportions of weight and volume with other atoms. He thereby changed chemistry from a qualitative to a quantitative science, giving an impetus to research which at last promises to bring us within sight of the fulfilment of Faraday's prophecy, that "in the end there will be found one element with two polarities." Many workers followed on the lines laid down by Dalton; notably Prout, who formulated the theory that the atomic weights are multiples of the atomic weight of hydrogen, the lightest of the so-called elements, and which he argued might be regarded as the primordial element, the *materia prima*, from which the others are formed by successive condensations.

The researches of the past few years establish the fact that certain of the elements possess such strongly marked likenesses as to warrant their classification into groups, but these groups did not appear to be connected with one another, nor to have any relation to the far larger number of elements not falling into groups. Recently, however, a marked advance towards proof of the common origin of all the elements has been made, in which an English chemist (Newlands) led the van, but in which a Russian chemist (Mendelejeff) has outstripped him in showing that if the seventy "elements" which have thus far been discovered are arranged "in the order of their atomic weights from hydrogen as 1, to uranium, the heaviest, as 240, the series does not exhibit continuous advance, but breaks up into a number of sections, in each of which the several terms present analogies with the corresponding terms of other series. Thus, the whole series does not run a, b, c, d, e, f, g, h, &c., &c., but a, b, c, d; A, B, C, D; α, β, γ, δ, and so on, in recurring similarities." In this we have a *periodic law*, as it is called, which embraces all the elements according to the increasing value of their atomic weights, and which has restored to their rightful place in the succession certain elements for which no place in any of the series of groups could be found. More than this, and as evidencing to the fruitful play of the imagination, Mendelejeff, finding certain gaps between neighbouring elements, pointed out that they could only be filled by elements possessing chemical and physical properties which he accurately specified. And, sure enough, some of these vacancies have been filled by the discovery of elements with the properties which Mendelejeff predicted they must possess. This is as interesting a romance as the discovery of Neptune, the existence of which, it will be remembered, M. le Verrier and Professor Adams independently deduced from the anomalous movements of Uranus, and which "floated into the ken" of Dr. Galle at Berlin when he pointed his telescope to that part of the heavens where the mathematicians told him he would find the planet.

Commenting on this significant grouping of atoms, Professor Huxley, in his masterly survey of the progress of science in Mr. Humphry Ward's "Reign of Queen Victoria," says that it "is a conception with which biologists are very familiar, animal and plant groups constantly appearing as series of parallel modifications of similar yet different primary forms. In the living world, facts of this kind are now understood to mean evolution from a common prototype. It is difficult to imagine that in the not-living world they are devoid of significance. Is it not possible, nay probable, that they may mean the evolution of our 'elements' from a primary form of matter? Fifty years ago such a suggestion would have been scouted as a revival of the dreams of the alchemists. At present it may be said to be the burning question of physico-chemical science." And although no known energy heat that we can apply can separate any one atom into two, so that, as Dalton said, "no man can split an atom," we do not any longer speak of it as indivisible; all that can be said is that it has not yet been divided. That triumph awaits the chemistry of the future, and, when it is accomplished, the witness to the unity of the universe will be complete.

SHAKESPEARE AND BACON.*

By Benvolio.

"Though in thy stores' account I one must be;
For nothing hold me, so it please thee hold
That nothing me, a something sweet to thee:
Make but my name thy love, and love that still,
And then thou lov'st me, for my name is *WILL*."
Shakespeare's "Sonnets."

IN the "Nineteenth Century" for May 1886 an article appeared, in which a particularly preposterous development of the absurd Baconian theory of Shakespeare's plays was brought before the notice of Shakespearean students. We were assured that Mr. Ignatius Donnelly had discovered a cipher which had been craftily concealed within the folio edition of Shakespeare's plays, published after his death, and that in two or three months Mr. Donnelly would publish most surprising readings from the cipher. It does not seem that Shakespearean scholars were very much impressed. The best of them all, the late Dr. Baynes, editor of the "Encyclopædia Britannica," brought out half a year later an appreciative essay on Shakespeare, in which the Baconian theory was not even mentioned. And now Mr. Donnelly feels moved to repeat his assertions and to renew his promises.

In the first place, Mr. Donnelly has persuaded himself that Bacon took special interest in planning cipher systems by which records, such as could not safely be published,

* It is proposed to enter presently in these columns into the study of Shakespeare's nature as unconsciously disclosed in his works, whence also something of his life may perhaps be gleaned. The present essay, from the American "Forum" for October, may be regarded as fitly introducing the series of papers thus planned, Mr. Donnelly's foolish assault on the character and capacity of Shakespeare having in reality (absurd though it was in itself) started the line of thought of which the proposed series of papers will be the outcome.

might be preserved ready for reading, when, later, the key of the cipher was indicated. As a matter of fact, when dealing with "Writing," in the third division of his section on the "Organ of Speech" in his "De Augmentis," Bacon does describe a cipher of his own, which he invented in his youth, at Paris.* But Mr. Donnelly would have done well to notice that Bacon very definitely expresses his opinion about the qualities which a good cipher should possess. If Mr. Donnelly is right about the imagined cipher in the folio edition of Shakespeare, that cipher would be a very bad one, according to Bacon's ideas. "A good cipher," says Bacon, "must absolutely elude the labour of the decipherer," which the folio cipher has failed to do; and "it must yet be commodious enough to be readily written and read," whereas the cipher in the folio, according to Mr. Donnelly's own account of it, would have been fearfully difficult to write, and, as we can judge from the long delay of Mr. Donnelly's promised volume, and the small portion of the folio which he promises to decipher at first, the cipher is singularly difficult to read, even when its key has been discovered.

Passing over the overwhelming antecedent improbability that Bacon ever wrote a line of the Shakespeare plays, and the extreme unlikelihood that he would have devised so cumbrous a cipher (when a few documents left to be read fifty years or so after his death would have served the full purpose attributed to him), let us consider the evidence in detail.

Mr. Donnelly believes that the words of a hidden narrative were to be placed in such situations in the plays, as printed in the folio edition, that when the key was discovered the whole narrative could be put together, Bacon's authorship proved, and many unsuspected details of his life, and of the history of his period, disclosed. It is not easy to present with gravity the first part of the evidence on which this idea, antecedently so absurd, has been based. We are told that Bacon felt sure some student of Shakespeare would notice the frequent appearance of the words "Francis," "Bacon," "Nicholas," "William," "Shakes," "peere," "Shake," "speare," "spurs," "spheres," &c., in the historical plays; he knew further that the ingenious student of the future would immediately associate this observed fact with what Bacon had said about ciphers in his "De Augmentis," and, "having once started upon the scent, would never abandon the chase until he had dug out the cipher." The mixed metaphor is Mr. Donnelly's own.

But now see what curious proof of the existence of special peculiarities Mr. Donnelly has obtained. On page 53 of the "Histories" the word "Bacon" is the 371st from the top of the first column. Now there are seven italic words in that column. Multiply 53 by 7 and we get 371! On page 54, we find in the first column twelve words in italics. Multiply 54 by 12 and we get 648. Counting words from the top of the first column of page 54, we come to the word "Chuffes," in which even the lively fancy of a Donnelly cannot recognise any specially Baconian significance. It is rather hard, because the word "Bacon" occurs in the poetic compound "bacon-fed," thirty-two words earlier, and the word "Bacons," eight words further on.†

But Mr. Donnelly is not to be foiled by such a difficulty as this. Nay, he does not even mention it. Not finding anything to suit him on page 54, from which he had obtained the number 648, he turns back to page 53, without any reason assigned, and finds there the 648th word to be Nicholas—the Christian name of Francis Bacon's father. Even this marvellous result is only obtained by humouring the count. Mr. Donnelly admits that in this case words in brackets are to be omitted; and he must have some system of counting hyphenated words as one or two to bring out the desired result, or else such words as "'twere" for "it were" "a clocke" for "o'clock," and so forth, may be considered single or double as required.

Mr. Donnelly appears not to have been deterred by the failure of the method on page 54 from trying it on page after page, until at last, coming to page 67, he obtained something like a success—at least, to one so sanguine as himself. There are six italic words in the first column of page 67, 6 times 67 is 402, and the 402nd word on page 67 is "S. Albones," for "St. Albans," the place from which Bacon's title was taken. It rather impairs the value of this coincidence that if we are to take "S. Albones" thus as one word, so also should we take "S. Nicholas" as one. Mr. Donnelly has already taken just so much of this word as his case wanted; though, indeed, the iniquity which his theory attributes to Bacon, Shakespeare, Ben Jonson, and all others supposed to be in the plot, is so great, that he might well have taken the whole word—the name of the patron saint of those who commit rascality under cover of darkness—as specially belonging to the imagined cipher system. What he does in one case he should do in the other, only it would not suit his theory to have only "Albones."

I cannot weary the reader with examples of other methods of counting, invented by Mr. Donnelly to serve as occasion may require. It must be admitted that it is not his fault that no constant rule will serve him. Sometimes he must be free to multiply by the number of words in brackets instead of by the number of words in italics; sometimes to count from the top of the page itself, sometimes from the page before, sometimes from the page after; sometimes to count hyphenated words as single, sometimes as double, and so on. But I cannot follow him in detail, because no sensible reader can be expected to examine many of these inanities. Suffice it that among the words found by these multitudinous devices are "volume," "maske," "his," "greatest," "therefore," "shown," "image," "but," "own," and others, which assuredly no one but Mr. Donnelly will regard as amazingly significant.

One case only will I cite as illustrating Mr. Donnelly's singular readiness to be startled into conviction by casual coincidence. The reader should carefully note each detail separately, for there is absolutely nothing to connect them together. The number of page 75 multiplied by 12, the number of italics in the first column of another page, page 74, gives 900; and the number of page 76, multiplied by 11, the number of words in brackets in the first column of the same page 76, gives 836. Now counting from the top of the first column of page 74, omitting words in brackets, and counting the hyphenated words no longer as two words but as one, the 836th word will be found to be the 304th word of

* The cipher is interesting as anticipating the Morse alphabet, in so far as it depends on the varied placing of things of two different kinds—Italic letters and Roman letters in the case Bacon describes.

† I venture to offer Mr. Donnelly a hint, just here. May not these numbers, 32 and 8, be highly significant? Eight is contained four times in thirty-two. Now the word "Bacon" appears only four times in all Shakespeare's plays; and in two of these cases it appears not simply as "Bacon," but in one place as "Bacons" and in the other as part of the compound "bacon-fed." Now, applying a certain rule we imagine we have discovered, we fail to get any Baconian word, but we find two of the "Bacons" out of all the four in Shakespeare on either side of the word we have lit upon—one of them four times as far from it as the other. "Can this be accidental?" Mr. Donnelly should have inquired. Are not the chances thousands to one against the occurrence of so many twos and fours in connection with the word "Bacon"? If any doubt can remain on this point, it ought to vanish when we notice that the numbers 8 and 32 are each multiples of four and powers of two, these powers being also four less one and four plus one respectively. One can go on with such drivel, however, indefinitely.

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page 75, and is the amazingly significant word "found"! Beginning from the top of page 75, and counting onward in the same way, the 836th word is "out." But counting from the same points, taking in the words in brackets, and counting each hyphenated word separately, we find the same two words, "found" and "out," each as the 900th instead of the 836th word in its respective count!

On attaining this result, about as interesting as the discovery that the number of words in one of the books of the "Iliad" or "Odyssey" chances to be a perfect square, Mr. Donnelly exclaims: "Can any man believe that this is the result of accident? It could not occur by chance one time in a hundred millions. The man who can believe this is the result of chance would, to use one of Bacon's comparisons, 'believe that one could scatter the letters of the alphabet on the ground, and they would accidentally arrange themselves into Homer's Iliad.'" It must be admitted, however, that the error into which Mr. Donnelly falls as to coincidences of the sort is a common one. "What strange hands were dealt us," someone will say at whist; "I wonder what the chances were that those particular hands would be dealt: millions to one against, I should imagine!" The answer is that the odds were more than six hundred and thirty-five thousand millions to one against those exact hands, but that the question of chance is not affected. Every set of hands at whist might be regarded as a marvellous coincidence if we viewed the matter in that way. The real question is, What is the probability that in a given set of hands odd coincidences may be found, if we look carefully for them? and the answer is, that nearly always you can find such coincidences if you look for them with sufficient patience. And so it is with such counting of pages, italics, brackets, words, hyphens, &c., as Mr. Donnelly has fruitlessly undertaken. You are bound to find hundreds of such coincidences as he notes for marvels.

But we must notice also the strange reasoning by which Mr. Donnelly has persuaded himself that the text of the folio has been altered—"twisted," as he says, "to conform to the requirements of a mathematical cipher"—though Bacon was weak indeed in mathematics. Mr. Donnelly notes the appearance of italicised words, hyphenated words, and words in parentheses, which he insists on calling brackets, and represents as brackets when quoting. He does not seem aware of the fact that when the folio was printed it was the custom to italicise all proper names as they are italicised in the folio, to hyphenate all connected words, such as "lean-on," "get-over," "find-out," &c., and to use parentheses to inclose words presenting an interjected expression or thought, which in modern printing would only be inclosed between commas. (I prefer this old usage myself.)

To show how ready Mr. Donnelly is to imagine peculiarities where in reality all is in order, I note that he regards the lines

> "You are too great to be (by me) gainsaid,"

and

> "I cannot think (my Lord) your son is dead,"

as printed in an unusual and unnatural fashion; and he asserts that in the first part of "Henry IV." such phrases are not so printed. Yet had he but turned for comparison to the most striking of all those passages in the first part of "Henry IV." which relate to the Percy plot, he would have found the lines

> "This bald, unjointed Chat of his (my Lord)
> Made me to answer indirectly (as I said),"

precisely matching the cases which he deems so strange. It would be impossible to convince Mr. Donnelly that lines which he quotes as strange, contorted, confused, &c., are perfectly natural and especially Shakespearean; for he manifestly has not the slightest germ of the faculty which enables the critic to recognise at once the touch of Shakespeare's hand. He finds such expressions as "the dole of blows," walking "o'er perils on an edge" (compare "on the unsteadfast footing of a spear"), and so forth, altogether unnatural. He cannot even understand so simple a passage as—

> "The lives of all your loving complices
> Leane-on your health, the which, if you give o'er
> To stormy passion, must perforce decay;"

asking gravely how lives can decay, when Shakespeare clearly speaks of Northumberland's health decaying. But the greatest absurdity of all, in this connection, is Mr. Donnelly's elaborate mystification in regard to the lines

> "Or what hath this bold enterprise bring forth,
> More than this being which was like to be?"

Of course, "bring" is a misprint for "brought": the folio is far from being so carefully printed that that need astonish us. But Mr. Donnelly says the line "more than this being which was like to be," reads like an extract from Mark Twain's recent essay on "English as She is Taught." Yet, even as Mr. Donnelly misquotes the line, it should perplex no one. "What," asks Morton, "hath this bold enterprise brought forth, more than this condition of affairs which was likely in any case to have come to pass?" It should be noticed, by the way, that in the folio the line runs—

> "More then that Being, which was like to be?"

"Then" is equivalent to "than," and "that" slightly alters the sense; but the point to be noticed chiefly is that the capital "B" marks the word "Being" as a noun (condition, state of affairs), and not the participle for which Mr. Donnelly has manifestly taken it. The comma, also, after Being, makes the sense obvious. The meaning of the passage should be clear, however, without this evidence from the folio itself.

With a lively imagination for the suggestion of impossibly ingenious cipher systems, and complete freedom from such restraints as Shakespearean scholarship would impose, Mr. Donnelly may read almost anything in the folio edition of Shakespeare. He can make his own history of Bacon's secret Shakespearean life, and find every item of it in the plays as printed in that edition. I have little doubt that in this way he has found already, to his own satisfaction, what would be most surprising if really regarded as the work of Bacon. The first sentence he publicly claimed to have read would of itself astound any one who had made any acquaintance with Elizabethan literature. It begins, "I was in the greatest fear that they would say that the image," &c. He might almost as reasonably have made Bacon say, "It was too awfully awful to think that they would say that," &c. Not a sentence published between the years 1550 and 1650, or even until later than 1750, resembles in structure the sentence attributed by Mr. Donnelly to Bacon, a master of the tersest style of which the English language is capable. Mr. Donnelly's marvellous first-fruit was not only a sentence of purely nineteenth-century English, but a very clumsy example even of that.

Finally, Mr. Donnelly pretends to wonder that Englishmen should be wroth with him for striving (as he puts it) to pass the fame due to the author of the plays from one celebrated Englishman to another. The pretence is twofold. No Englishman that I have ever heard of, and no American of English descent (to whom Shakespeare's fame must be as dear as to the native-born Englishman, since birthplace is the merest accident), has ever viewed the Baconian theory of Shakespeare's plays with any feeling resembling wrath. A foolish fancy like that theory may provoke a smile, but certainly no anger; and our amusement can only be intensified by such an amazingly absurd extension of the

theory as Mr. Donnelly has wandered into. But the theory, could it be established, would not hand the fame of "gentle Will Shakespeare" to Bacon, the keen logician and potent reasoner; it would bring discredit to the names of both, as also to others esteemed for varied attainments and qualities, whom the Baconian theory associates with Shakespeare and Bacon in a cowardly and shameful plot.

Gossip.

BY RICHARD A. PROCTOR.

IN this number are begun several of the subjects promised in the announcements made respecting the eleventh volume of KNOWLEDGE. How much more might be done if the public would give one tithe of the support to a magazine relating to science which they will give to a magazine incorporating sensation stories by tenth-rate novelists, few, save the proprietors and publishers of scientific magazines, can guess. Every month's issue of a magazine like this involves a sacrifice of time, labour, and money, entirely inconsistent with the sound old saying that "the labourer is worthy of his hire."

* * *

By an odd coincidence, just after reading through, in KNOWLEDGE for September, an article in which true loyalty and false loyalty were contrasted, I opened the October issue of "The Forum" at the stupendously silly—unless it is to be considered the bitterly sarcastic—article by General Lord Wolseley on "Queen Victoria's Reign."

* * *

General Wolseley found in the Jubilee clatter evidence of love for a family representing "all that we most delight to dwell on in our history," "the heirs of our lion-hearted Richard" (recognised by history as one of the coarsest, and at heart most cowardly of ruffians), "of our Henries of York and Lancaster" (the Henries of York are unknown to history, and only one Henry of Lancaster was even respectable in character and conduct, the fourth Henry being a treacherous murderer, and the sixth a nonentity), and "of our own great Tudor Elizabeth." Queen Victoria's kinship to Elizabeth is remote—to say the least—considering that we have to go back to Henry VII. before we can advance down the line of descent to the one Stuart King of England, through whom the Hanoverian line claims kinship with the earlier monarchs of this country. But when we consider that less than a thousandth part of the Queen's blood came from that Stuart monarch, it is rather absurd to grow enthusiastic about the attributes of the present royal family. Ask Mr. Francis Galton how much of the old fighting and ruling qualities of the Norman kings could have been handed down by direct hereditary descent even to the later Plantagenets, and his answer will hardly favour the idea that, for example, George III., whom Wolseley openly ridicules as a blundering old ass, or George IV., whom sober history recognises as a brainless and heartless humbug, could have inherited any exceptionally kingly attributes from those admirable ancestral plunderers. Loyalty like Wolseley's, which depends solely on the asserted amiability and good sense of the actual monarch, and openly despises her nearer ancestry, is not loyalty at all, even of the poor kind considered—and assuredly cannot be tinselled into better semblance by references to far-back ruffians palmed off in children's histories as gallant knights and able rulers.

* * *

It is not here and thus, however, that General Wolseley chiefly blunders in his easily explained enthusiasm. He deliberately quotes, as the most characteristic samples of true loyalty, conduct which in the selfsame breath he calls superstitious or silly or ignorant (or all three). He was "much struck" by "a newspaper description of the unveiling of the Queen's statue in India," telling how the ignorant natives, regarding it as a kind of idol, "rushed forward and kissed the feet" of it! "The simpler the nature of the peeple," he justly says (he *must* mean his whole article ironically), "the more unquestioning is their religious faith and that loyalty which is akin to it." Then he tells how a little girl, after the Hyde Park *fête*, went home and told her mother she had seen a balloon go up which had taken "the Queen to heaven." "The idea may" (*sic*) have been "silly in itself, but it signified a train of reasoning in which loyalty was evidently a prominent element." Could anything more sarcastic have possibly been said by the keenest advocate of true *versus* false loyalty?

* * *

Next, the gallant opponent of the superstitious but most loyal followers of the Mahdi tells us how "a poorly-clad nursemaid, pushing a perambulator before her through the crowds in the 'East End,'" expounded *her* ideas about the Queen—and he grows enthusiastic over her utterances: "This simple nursemaid," he says, "like millions of other people, was imbued with the species of hero-worship which in monarchies is known as loyalty."

* * *

Then he somewhat liberates the feline from its encompassment by dwelling on the fact that "personal devotion to the sovereign is more apt to be" (or to seem) lively, "when all favours, rewards, and punishments emanate directly from the throne: the less this is the case, the feebler we should expect to find those feelings of which loyalty is compounded." Possibly General Viscount Wolseley has had occasion to appreciate both that kind of gratitude which has been described as "a lively sense of favours to come," and that anxiety as to punishment which naturally, as he says, suggests the sense of personal—aye, intensely personal—loyalty. If so, he may well ask, consciously and anxiously, whether the spirit which teaches men to despise adulation of the powerful and to advocate true self-respecting loyalty, "confers a boon upon mankind" (as represented by number one) in "seeking to eradicate loyalty" (of the false kind) "from the human heart."

* * *

But if this is the way in which such friends of loyalty, falsely so called, defend it, such loyalty, could it but speak for itself, might exclaim "Save me from my friends,"—with almost as much reason as Gordon at Khartoum.

MYTH, RITUAL, AND RELIGION.*

THE science of comparative mythology is fortunate in having had the methods of its expositors subjected to severe tests at what is still an early stage of its history. We owe no small debt to the scholars who rescued materials which imbed men's thoughts while at low levels of culture from the hands of dictionary-makers and allegorists, and who made plain its deep and long-hidden significance. But this must not blind us to the defects of their method, which, as our readers scarcely need to be reminded, explains the repulsive and ludicrous features in the myths of higher races as due to what Professor Max Müller calls a "disease

* "Myth, Ritual, and Religion." By Andrew Lang. Longmans & Co.

of language," to the forgetting of the purer primitive meaning underlying the names of the gods and heroes of mythology, and which it is contended, by an appeal to their supposed etymologies, were, in the first instance, names of the sun, the dawn, the thunder, and so forth.

The arguments in support of this, presented in attractive guise and with much show of reason, in Professor Max Müller's well-known essay, reprinted in the first volume of his "Chips," and elaborated in Sir George Cox's "Mythology of the Aryan Nations," have held the field for some years, being only now and then attacked by skirmishers, or by the light artillery of clever parody. But of late a growing feeling of the insufficiency of a method which rests chiefly on evidence from words as to whose meaning experts differ, and which, moreover, interprets only the myths and rituals of ancient and modern civilisations, while ignoring or undervaluing those of savage races, has arisen, the result of which is to condemn that method as untenable in the main, and as applicable only to a very small portion of the great body of myth.

The impetus to this discrediting of etymology as the sole key to interpretation came from a comparison of the myths of the higher with those of the lower races, which brought out the fundamental likenesses between them in the coarse and wild elements common to each. Anthropologists explain the presence of these elements in Greek, Vedic, and other mythologies, as survivals from the lower culture out of which Greeks and Brahmans have emerged. They are the old Adam which has never been cast out. Like the ancestral history of the type which the embryo repeats in its advance from the egg to the full-grown state, myths preserve traces of the intellectual and spiritual types in which their earliest forms were cast, and thus add their witness to the unity and continuity of history.

Thus viewed, myths, rituals, and religions, wherever found and in whatever refined or unrefined connection, fall into their related place in the general march of man's development. It is of this sound and verified method of anthropology—which has no limitation of race or zone—that Mr. Lang is the most prominent and cultured exponent. He can claim for it, as his letter to Eusebius in the delightful "Letters to Dead Authors" shows, a venerable antiquity, since the learned Bishop of Cæsarea, in treating of the "pagan" mythologies, argued that "they descend from a period when men in their lawless barbarism knew no better than to tell such tales. Ancient folk in the exceeding savagery of their lives made no account of God, but betook them to all manner of abominations. Growing a little more civilised, men sought after something divine, which they found in the heavenly bodies. Later they fell to worshipping living persons, especially medicine men and conjurors, and continued to worship them even after their decease, the Greek temples being really tombs of the dead. (Which, by the way, applies to every Roman Catholic church, since, according to Papal traditions, unconsciously conserving the barbaric worship of ancestors, there can be no altar where there are no relics.) Finally, the civilised ancients, with a conservative reluctance to abandon their old myths, invented for them moral or physical explanations like those of Plutarch and others earlier and later."

Mr. Lang's diligence has also unearthed an essay by Fontenelle, a nephew of Corneille, which was published in 1758, and in which he explains the absurdities of the old mythologies as the legacy of the savage and ignorant ancestors from which every civilised race is descended. He "concludes that all nations made the astounding part of their myths while they were savages, and retained them from custom and religious conservatism." This could not be better or more briefly put.

The space given to a quarterly review article would only suffice to furnish an outline of the profusion of illustration from ancient and modern sources with which Mr. Lang supports his general thesis. The present volumes—as easy to read as a novel, and far more entertaining than nine-tenths of the novels published nowadays—are a careful and elaborate restatement of all that Mr. Lang has hitherto published in fugitive form or in the more collected essays comprised in his earlier book on "Custom and Myth," which was the subject of a lengthy notice in this journal three years ago.

Brushing aside the notion that even in the lowest and crudest myths we touch the beginnings of thought, Mr. Lang gives a rapid but sufficient survey of the interpretation of ancient and modern mythologists, wisely transferring his answer—complete and crushing as it is—to the objections raised against the anthropological methods, chiefly by Professor Max Müller, to an appendix. The body of the book is thus relieved of contentious matter, and filled with examples drawn from the lower and the higher culture, bringing out with clearness the remarkable coincidences between the myths of Greeks and Bushmen, of Finns and Kaffirs, of Aztecs and Zulus. Some prominence is given at the outset to the widespread—we may say universal—attribution of life and personality to everything by savages, and to their belief in descent from sun, animal, or plant, as the key to their theologies, rites, and customs. Very much remains unexplained, but the agreement of the evidence drawn from races between whom no intercourse has taken place since their first dispersion leaves little doubt that such practices as the prohibition against marriage between members of the same tribe-name or totem, and against eating the animal which gives its name to the totem, arise from belief in the near kinship of man and brute.

Mr. Lang's skill in disentangling an intricate subject from the webs of theory-spinners is markedly shown in his two chapters on the gods of the Indian Aryans and on the mythology of Egypt. His sanity of view is apparent in the conclusions at which he arrives concerning the latter, and which agree with the general conclusions reached throughout the volumes. "In Egypt, as elsewhere, a mythical and a religious, a rational and an irrational, stream of thought flowed together, and even to some extent mingled their waters. The rational tendency, declared in prayers and hymns, amplifies the early belief in a protecting and friendly power making for righteousness. The irrational tendency, declared in myth and ritual, retains and elaborates the early confusions of thought between man and beast and God, and between things animate and inanimate. On the one hand, we have almost a recognition of supreme divinity; on the other, savage rites and beliefs shared by Australians and Bushmen. Egyptian religion and myth are thus no isolated things; they are but the common stuff of human thought decorated or distorted under a hundred influences in the course of unknown centuries."

Mr. Lang has an easy task in explaining why certain groups of myth, even those of whole races, as Finns and Scandinavians, should be excluded, at least, for the present, unless his volumes are to grow to unwieldy size. But he gives no reasons for the omissions, here and there, which betray a reluctance to include the myths and legends of Judaism and Christianity as due to the like causes which explain the myths and legends of other religions. Silence upon this subject does harm in fostering prejudices which are strengthened when the mythologies and cosmogonies of a Semitic tribe are treated as an integral part of sacred writings into which there enter elements as coarse and crude as those which are found in Vedic hymns and savage legends. For example, Yahweh (Jehovah) smells the sweet

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savour of Noah's sacrifice (Genesis viii. 21), and Indra eats the flesh of bulls ("Rig-Veda x. 28, 3); Yahweh-Elohim creates man out of moist earth (Genesis ii. 6, 7), and the Australian Pund-jeh makes two clay images of men and breathes his breath into them.

We can only conclude this inadequate notice by saying to the solar mythologists that if, after honestly weighing the arguments advanced in this important work, they believe not Mr. Lang and his cloud of witnesses, "neither will they be persuaded though one rose from the dead."

Reviews.

In Cheviots Glens. By JANE T. STODDART. (Oliphant.)—This is a very pleasantly written story of modern life on the Scottish Border. The scenes and characters are not ostensibly sketched from life; but, presuming that they are, none of the originals would, we think, have any good reason to complain that Miss Stoddart had not done them justice. It is a little odd to find the shepherd's son marrying the squire's daughter, even though he was a "meenister"; but we suppose "they do these things better" in Scotland, or oftener, at any rate, than we do here. We have spent a very pleasant hour "In Cheviots Glens," and hope to meet Miss Stoddart in the neighbourhood again.

My Microscope; and Some Objects from my Cabinet. By a QUEKETT CLUB-MAN. (Roper & Drowley, Ludgate Hill. 1s. 6d.)—The Quekett Club, to which this little work is dedicated, is not likely to increase its credit largely by the connection. We are totally unable to see the *raison d'être* of the book. Gosse's "Evenings at the Microscope" has been cheaply reprinted. Houghton's "Microscope and the Wonders it Reveals" is selling at less than a shilling, and there are a dozen other little works which will each tell the beginner twenty times as much as he can learn from the pages before us. It is an elegantly got-up little book, thick paper, large type. The binding is good. The printing is well done. But that is all we can say of praise. The illustrations are few and poor. One roughly depicting a hydra is suggestively labelled on the back "a monster." There is a group of diatoms fairly well drawn in the chapter on "A Skeleton." But the text, instead of giving even their names, indulges in a few generalities on *Aulacodiscus orientalis*, which is not figured. The other chapters treat on "An Eye," "A Wing," "A Slice of Rock," &c., and make a few remarks on a spider, a butterfly, and so forth. The work contains 78 pages. Eighteen are absolutely blank; eleven more, including the dedication and table of contents, share seventy-five words among them. In conclusion we can only regret that any one professing to love the microscope can say so little for it at the price.

A Junior Course of Practical Zoology. By A. MILNER MARSHALL, M.D., D.Sc., M.A., F.R.S., assisted by C. HERBERT HURST. (London: Smith, Elder, & Co. 1887.) As an introductory text-book for the student of zoology, the work of Dr. Marshall and Mr. Hurst leaves little or nothing to be desired. Written with scrupulous care by men personally thoroughly familiar with the objects and processes they describe, and illustrated by woodcuts which, if few, are excellent, the student who will sedulously work through the graduated series of dissections so carefully and minutely treated of in it, will have attained a very considerable knowledge indeed of animal morphology. Our authors begin with the most rudimentary forms of life, the amœba, paramecium, vorticella, &c. (the so-called "infusoria"); and then ascend through the hydra, the liver-fluke of the sheep, the leech, earthworms, and the like; the crayfish, cockroach, lancelet, and dogfish, to the rabbit, fowl, and pigeon. An immense amount of honest work is embodied in the volume before us, which will doubtless speedily attain high rank as a handbook in schools of anatomy and physiology.

Lessons in Elementary Mechanics. By W. H. GRIEVE, P.S.A., late R.N. (London: Longmans, Green, & Co. 1887.)—In simple language, and with an abundant supply of illustrative woodcuts, Mr. Grieve treats of the six "mechanical powers" of the old books on Natural Philosophy: the lever, the wheel and axle, the pulley, the inclined plane, the wedge, and the screw; as also of liquid pressure, the hydrostatic press, liquids under the action of gravity, and the parallelograms of forces and of velocities. The examples selected are derived from objects in familiar use, and the pupil must be abnormally stupid or idle who fails to understand Mr. Grieve's very plain exposition of them. He has done his work well.

Handbook of Practical Botany. By E. STRASBURGER. Edited from the German by W. HILLHOUSE, M.A., F.L.S. (London: Swan Sonnenschein, Lowrey, & Co. 1887.)—We are glad to welcome Professor Strasburger's admirable manual of structural and physiological botany in its English dress, supplying, as it does, a want in our microscopical literature. Nor has our examination of the work seemed to furnish much, if any, justification for the apologetic tone in which Mr. Hillhouse speaks of his translation, since this appears, as far as we can judge, to render the sense, and even in some sort the diction, of the original very well indeed. Famed as Herr Strasburger is as a microscopical manipulator and observer, the exhaustive mass of detailed description of the dissection and preparation of plants for examination which this volume contains cannot fail to be of the greatest interest and use to the vegetable histologist and physiologist. Nothing is omitted which can facilitate the processes described, and profuse illustration supplements directions themselves of the most explicit character. There are careful tables of the plants used for study, the re-agents employed in their examination, preparations for mounting them, &c. In short, this is a book which every microscopist ought to have, and every botanist must have.

An Introduction to Machine Drawing and Design. By DAVID ALLAN LOW. (London: Longmans, Green & Co. 1887.)—This very practical little book is the work of a man thoroughly familiar with the subject of his descriptions, and who further possesses the faculty of imparting his own knowledge easily and pleasantly. It should be studied not only by the young engineer, but by the fitter, turner, and, in fact, everybody who has to make drawings of machinery or work from them.

The Problem of Evil. By DANIEL GREENLEAF THOMPSON. (London: Longmans, Green, & Co. 1887.)—Mr. Thompson, in the very able and important work before us, investigates the nature and origin of evil, and essays to point out the most hopeful means for its elimination. In doing this, he perforce discusses what he calls "the Great Theological Superstition," and paints in vivid colours the horrible immorality of the doctrine weekly taught from thousands of pulpits. He discusses at length the suggested methods (social, political, and ecclesiastical) for reducing evil to a minimum, which have been and are still advanced, and shows trenchantly the fallacies which underlie them all. The conclusion at which he arrives is that the elimination of evil, and consequent amelioration of mankind, can only be effected by—first, aiming at the minimum of extrinsic restraint and the maximum of liberty for the individual;

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and secondly, aiming at the most complete and universal development of the altruistic character. We will not diminish the pleasure with which the reader will peruse this volume by any more detailed analysis of its contents. Suffice it to say that Mr. Thompson has made a real and enduring contribution to ethical philosophy.

Labour, Leisure, and Luxury. By ALEX. WYLIE. (London: Longmans, Green, & Co. 1887.)—To any philanthropist who is anxious to celebrate the Jubilee year by doing permanent good to a large and important section of his fellow-countrymen, we would commend the idea of the circulation, broadcast, among the working population of Great Britain of Mr. Wylie's most excellent little volume. As an antidote to the dangerous sophisms and interested rant of Champion, George, Hyndman, and Co., it would be difficult to find anything to surpass it.

Manual of Bacteriology. By EDGAR M. CROOKSHANK, M.B., F.R.M.S. Second Edition. (London: H. K. Lewis. 1887.)—Very appreciable justification of the praise which we bestowed upon the first edition of Mr. Crookshank's beautiful work (on page 134 of our ninth volume) is to be found in the fact that after so comparatively short a lapse of time, a second has been called for. Our author has taken advantage of this to add considerably to his work in its original form. The new chapters on Antiseptics and Disinfectants, and on Immunity, possess high interest for others besides the mere specialist. It would be impossible to praise too highly the very beautiful plates with which the volume is so liberally illustrated.

A Photograph, and How to Take It. By "ONE WHO KNOWS." (London: E. G. Wood. 1887.)—This is a treatise on photographic manipulation for the very beginner, and the instruction it contains is given in such simple language as to be intelligible to the meanest capacity. It is followed by a catalogue of apparatus and materials sold by its publisher.

Tips in Algebra, by Rev. A. D. CAPEL, M.A. (London: Joseph Hughes. 1887); *Questions in Psychology, Ethics, and Metaphysics*, by F. RYLAND, M.A. (London: Swan Sonnenschein, Lowrey, & Co. 1887); *Chemistry for Beginners*, by R. L. TAYLOR, F.I.C., F.C.S. (London: Sampson Low, Marston, Searle, & Rivington. 1887); *Descriptive Geometry and Graphic Arithmetic*, by W. S. BINNS (London: Simpkin, Marshall, & Co.); *Euclid's Elements of Geometry*, Book I., arranged by A. E. LAYNG, M.A. (London: Blackie & Son).—The somewhat heterogeneous assemblage of books whose titles head this notice possess one attribute in common, and that is that they are one and all written as aids to the miserable examinee in that delightful system now in vogue for obtaining public servants which we have so slavishly copied from the Chinese. They are worthy of being devoted to more useful and laudable ends than that of being employed as mere cram books. To take a single illustration, Mr. Binns's "Graphic Arithmetic" will come almost as a revelation to a very large number indeed of his readers.

PHOTOGRAPHY.—Messrs. Marion & Co., of Soho Square, London, have just introduced a universal ten-per-cent. solution for developing, which promises to be of great assistance to amateur and professional photographers. The hitherto complicated formulas for developing plates manufactured by different firms may now be dispensed with, as everything is made easy to the operator by the use of this solution. Precise instructions are given on each bottle as to the quantities required for the plates of the principal makers in general use at the present time. The solution has simply to be poured into a bottle, no weights or measures are necessary, and no mistakes can ever occur if the directions fixed on the bottle are complied with.

THE FACE OF THE SKY FOR NOVEMBER.

WE are apparently approaching a period of sunspot minimum pretty rapidly, and days pass without visible signs of disturbance on the sun's disc. At the beginning of the month the zodiacal light still lingers to the south of east before sunrise. Map xi. of "The Stars in their Seasons" shows the face of the night sky. Minima of Algol ("The Stars in their Seasons," map xii.) will occur at 4h. 59m. P.M. on November 1st; at 9h. 52m. P.M. on the 18th; at 6h. 41m. P.M. on the 21st; and at other times more inconvenient to the amateur observer. Mercury comes into inferior conjunction with the sun on the 17th, and is an evening star after that date, but is very badly placed for the observer. Venus is a morning star, and is a brilliant object to the south of east about 3h. A.M. She still exhibits a beautiful crescent in the telescope. She is in Virgo ("The Stars in their Seasons," map v.). Mars is, for our present purpose, invisible; and Jupiter absolutely so. Saturn rises about 10 o'clock at night at the beginning of the month, and soon after 8h. P.M. at the end of it. His ring system will be seen to be very slowly closing up. He will be found a little to the west and north of δ Cancri ("The Stars in their Seasons," map iii.). Uranus cannot be seen until 1888; but Neptune is to be found in the blank part of the sky S.S.E. of the Pleiades ("The Seasons Pictured," map xii.). The moon enters her last quarter at 5h. 2m. in the afternoon of the 8th; is new at 8h. 8·4m. A.M. on the 15th; enters her first quarter at 10h. 43m. in the morning of the 22nd; and is full at 3h. 20·1m. P.M. on the 30th. She will occult seven stars at convenient hours during the month, besides others during the early morning ones. On the 6th, *g* Geminorum, a star of the 5½th magnitude, will disappear at the moon's bright limb at 10h. 34m. P.M. at an angle from her vertex of 338°. It will reappear at her dark limb at 10h. 51m. P.M., at an angle of 302° from her vertex. Before she rises on the 8th she will have occulted 7 Leonis, a 6½th magnitude star. Later it will reappear at her dark limb, at 11h. 8m. P.M., at an angle from her vertex of 182°. On the 18th, 33 Sagittarii, a star of the 5th magnitude, will disappear at the dark limb at 4h. 23m. P.M., at an angle of 38° from the vertex of the moon. It will reappear at her bright limb at 4h. 50m. P.M., at a vertical angle of 1°. Later on, at 6h. 2m. P.M., ξ² Sagittarii, of the 4th magnitude, will disappear at the dark limb at an angle of 103° from the moon's vertex, reappearing at her bright limb at 7h. 5m. P.M., at an angle from her vertex of 330°. On the 20th, B.A.C. 7202, of the 6th magnitude, will disappear at the dark limb at 4h. 46m. P.M., at a vertical angle of 124°. It will reappear at the bright limb of the moon at 6h. 2m. P.M. at an angle of 279° from her vertex. Next at 5h. 35m. P.M. B.A.C. 7209, of the 6½th magnitude, will disappear at the dark limb at an angle of 159° from the moon's vertex. It will reappear at her bright limb at 6h. 32m. P.M. at a vertical angle of 259°. And, finally, 19 Capricorni, a star of the 6th magnitude, will disappear at the dark limb of the moon at 8h. 30m. P.M. at an angle of 114° from her vertex, but she will have set prior to its reappearance at her bright limb. When these notes begin the moon is in Aries ("The Seasons Pictured," plate xxiii.), but at 10 o'clock to-night passes into Taurus. She is travelling through Taurus until 8h. P.M. on the 4th, when she reaches the boundary of the northern prolongation of Orion. It takes her just 12 hours to cross this, and at 8h. A.M. on the 5th she emerges in Gemini ("The Seasons Pictured," plate xxiv.). She is in Gemini until 4h. 30m. A.M. on the 7th, when she quits it for Cancer. She has completed her journey across Cancer by 6h. P.M. on the 8th, at which hour she enters Leo. Here she continues until 6h. 30m. A.M. on the 11th, when she passes into Virgo ("The Seasons Pictured," plate xxv.). Her passage through Virgo terminates at 3h. A.M. on the 14th, and she quits that constellation for Libra ("The Seasons Pictured," plate xxvi.). As she traverses Libra she comes, at 8h. P.M. on the 15th, to the western edge of the narrow northern spike of Scorpio, and when, by 4h. 30m. the next morning, she has traversed this, it is to emerge in Ophiuchus. She passes out of Ophiuchus into Sagittarius at 5h. P.M. on the 17th. She leaves Sagittarius and enters Capricornus at midnight on the 19th ("The Seasons Pictured," plate xxi.). She remains in Capricornus until 1h. A.M. on the 22nd, and then quits it for Aquarius, leaving Aquarius, in turn, for Pisces at 8h. A.M. on the 24th ("The Seasons Pictured," plate xxii.). In her journey through this great straggling constellation she enters on the confines of Cetus at 2h. A.M. on the 25th. At 9h. A.M. on the 26th she re-emerges in Pisces, only, however, once more to enter a part of Cetus at 9h. 30m. A.M. on the 27th. When she finally quits this, at 3h. A.M. on the 28th, it is to enter Aries ("The Seasons Pictured," plate xxiii.). At 5h. A.M. on the 29th she, for the second time this month, leaves Aries for Taurus, and has not completed her journey through the last-named constellation when our notes terminate.

Our Whist Column.

BY "FIVE OF CLUBS."

CLEVERLY BAD PLAY.

I HAVE given some attention for a long time past to the way in which good play at whist prevails over bad play; so that although for an evening, or even for several days, inferior play may secure the greater number of points, in the long run better play tells, and a certain percentage of advance infallibly shows itself ere many hundreds of points on either side have been scored. Nearly always, I have observed, the more scientific play (by which I do not refer to such details as knowing all the settled details of play—the "signal," the "echo," the "penultimate," and so forth, but following true strategic principles) succeeds most notably against those points on which the unscientific player is most apt to pride himself. Among these are:—(1) Playing for ruffs; (2) drawing two trumps for one when the enemy have declared great strength in trumps; (3) always declining to force the enemy; (4) putting off play by which a sure trick will go to the enemy—though nothing can prevent the trick so going, and it may be of essential importance to throw the lead into his hand at the moment; but there are many other clever dodges which are sure ways to failure in the long run, though they may turn out well perhaps five or six times in a score of trials.

But nothing shows the whist player better the value of sound play than to play for a while with a partner who does not understand the true principles of whist strategy, and puts trust in unsound dodges. Especially, while as yet he is unaware of his partner's foibles, does he suffer; for with a partner of known ineptitude he, of course, does not attempt the higher strategy, knowing that his plans will inevitably be knocked on the head.

Here is an example, which I will not put into tabular form, but describe it as it presented itself—unpleasantly—to my attention.

I was the original leader, and, on examining my hand, found that it contained the following pleasing set of cards (the ace of spades having been turned on my right):—

Spades—queen, ten, nine, four, three; *diamonds*—king, queen, knave, ten, eight; *hearts*—ace; and *clubs*—king, nine.

This was a charming hand to play, with a good partner, holding a fair supporting hand. The object to be aimed at, of course, is to bring in the long diamonds; and with five trumps, heart ace, and a guarded king in clubs, this seems very likely to be brought off. [The score was "One all."] Had I been free to look at my partner's hand in this case, I should have made sure of bringing it off with ease, unless he proved to be a very weak player. Let us see what happened:—

Of course I led a spade, the original fourth best trump; a small trump fell on my left; my partner played the knave; and the trump card, the ace, took the trick. My right-hand opponent, Z, led heart five; my ace took the trick; and four and three fell from Y and B, showing that two must be with Z; and, of course, three others, since he had led his fourth best. I now naturally led the spade queen, the three high trumps left in my hand being now in sequence. King took it on my right, my partner played a small trump, and Z renounced. This renounce looked, of course, unfavourable for my plans; in fact, I could tell that Y either held two more or three; if he only held two, however, I was still sure of success in bringing in my diamonds, provided my partner played rightly. Y returned his partner heart six (the lowest left, except Z's two), my partner took the trick with the king, Z played the two, and I discarded club nine.

At this stage the game was won. My partner held another small trump. We had already made two tricks to the enemy's two. If my partner led his trump I should have made two more tricks, extracting both Y's; have led diamond king, which Y would have taken with the singleton ace he held; and on his leading a heart (his only reasonable lead, but it was indifferent what he held, as my partner held club ace) I should have ruffed and made my four remaining diamonds, the last trick going to my partner's club ace. We should thus have made four by tricks.

But my partner deliberately committed the atrocity of forcing me (original trump leader though I was!) by leading a heart, on which Z put a little one, so that I had not even the satisfaction of ruffing the best heart, yet was unable to resist the force, as I could place two more hearts in Y's hand, so that the force could be repeated by him, and then by Z. (The inexperienced whist-player may ask how I knew Y had two hearts left: simply because my partner not leading the best, Z's play showed he knew Y could take the trick, which at once made his return of the lowest at the fourth trick indicate three hearts then in hand, for every good player returns the lowest of three, the highest of two.) I therefore ruffed, and now, play as I might, we could make only two by tricks. I led my diamond king; Y took with the ace, and, forcing me in hearts, remained with two trumps to my one. If I had taken out both his trumps, and then led diamond king, he would have brought in his partner's hearts, and we should have fared worse. If I had taken out but one of his trumps and then played the diamond king, he could have forced out my last trump with a heart, and on ruffing my diamond queen would have led his remaining heart.

My partner's reason for his atrocious play was that he saw a chance of giving me a ruff, and making one of his own trumps in a ruff. For the chance of one trick (my ruff, of course, was not a trick gained) he—the weak hand—ruined (instead of helping) the play of his strong-handed partner.

It is because of such iniquities as these that Deschapelles said good players of their own hand are *detestable* partners.

From some statistics I have been collecting now for two or three years I have been led to the conclusion that, while simply know-nothing partners lose about one trick in ten, clever players of their own hand lose about one trick in seven.

LONDON FOGS.—Mr. Ernest Hart, of the Smoke Abatement Institute, fears that London will always suffer from fogs, because it is placed in a river valley, on a clay soil, and is bordered on the Essex side by low-lying lands very imperfectly drained, and on the north side by the Harrow Weald. The fogs generated—the results of damp exhalations—are greatly aggravated by the parks, most of which require draining. But if the smoke is got rid of, the fogs will be much less dense.

Our Chess Column.

BY "MEPHISTO."

MATCH, BLACKBURNE *v.* GUNSBERG.

THE following games are the first four played in this match at Bradford:—

GAME I.—(FOUR KNIGHTS.)

WHITE Gunsberg.	BLACK. Blackburne.	WHITE Gunsberg.	BLACK. Blackburne.
1. P to K4	P to K4	19. P to KR4	P to Kt4 (1h.)
2. Kt to QB3	Kt to KB3		
3. Kt to KB3	Kt to QB3	20. KP × P	P × P
4. P to QR3 (*a*)	P to Q3	21. P to R5 (55m.)	P to KB5 (*i*) ! (1h. 15m.)
5. P to R3	B to K2		
6. P to Q4 (*b*)	Castles (*c*)	22. P to R6 (1h.)	R to Ktsq
7. P to Q5	Kt to Ktsq		
8. B to K3	Kt to Ksq (*d*)	23. Kt to R5	R to KB2
9. P to KKt4 (*e*) (17m.)	K to Bsq (24m.)	24. B to Q3	B to B4
		25. B × B (*j*)	R × B
10. Q to Q2	P to QB4	26. Q to Q3	R(B4) to Bsq
11. Kt to K2 (*f*)	P to QKt4	27. P to Kt6 1h. 25m.)	R × P (*k*) (1h. 44m.)
12. Kt to Kt3	P to QR3		
13. B to K2	P to Kt3	28. R × R	P × R
14. B to R6	R to Ktsq	29. Kt to R4	P × Kt
15. Castles, QR (*g*)	B to B3	30. KttoKt6(ch)	K to Ktsq
16. QR to Ktsq	R to R2	31. P to R7 (ch)	K × P
17. P to Kt5	B to KKt2	32. Kt to K7 (dis ch)	Resigns,
18. B × B (ch) (42m.)	R × B (*h*) (56m.)	(1h. 30m.)	(1h. 50m.)

NOTES BY MESSRS. BLACKBURNE AND GUNSBERG.

(*a*) The usual continuation here is B to Kt5, as played in the late Steinitz *v.* Zukertort match.

(*b*) The game develops itself into a *Philidor*.

(*c*) Mr. Blackburne now thinks P × P preferable.

(*d*) P to B3 would have been better.

(*e*) To prevent P to KB4.

(*f*) It would not have been advisable to castle too early on the Queen's side.

(*g*) Now White is sufficiently developed to castle on the Queen's side.

(*h*) If Kt had taken B, Black would have had two weak spots on KB3 and KR3.

(*i*) If P moved to K5, White gets a strong attack by P to R6, followed by Q to B3, &c.

(*j*) Kt × BP would also have been a strong continuation, leading to a good many pretty variations, *i.e.*, if 25. Kt × BP, B × B, 26. Kt to K6, to be followed by Q × B, &c.

(*k*) If P × P, White answers by P to R7, which would prove fatal to Black.

XUM

GAME II.—(SCOTCH GAMBIT.)

WHITE. Blackburne.	BLACK. Gunsberg.	WHITE. Blackburne.	BLACK. Gunsberg.
1. P to K4	P to K4	(1h.)	
2. Kt to KB3	Kt to QB3	21. Q to Bsq	QR to KB2
3. P to Q4	P × P	22. Kt to K2	Q × Q
4. Kt × P	B to B4	23. R × Q	Kt to K3
5. B to K3	Q to KB3	24. QR to Qsq	P to KR4
6. P to QB3	KKt to K2	25. P to KR4	Kt to R3
7. Q to Q2	P to QR3 (*a*)	26. B to K3	Kt to Kt5
8. P to KB4 (*b*)	B × Kt (*c*)	27. R × R	R × R
9. P × B	P to Q4	(1h. 45m.)	(59m.)
(1m.)	(10m.)	28. R to Q3	P to KKt3
10. P to K5	Q to Kt3	29. R to Kt3 (*h*)	P to Kt3
11. B to Q3	B to B4	30. R to B3	P to B4
12. B × B	Kt × B	31. P × P	Kt × B
13. Castles	Castles KR(*d*)	32. R × Kt	P × P
14. Kt to QB3	QR to Qsq	33. R to KKt3	P to Kt4 (*i*)
15. QR to QBsq (*e*)	P to KB3 (*f*)	34. Kt to B3	P to Q5 (*j*)
16. B to B2	P × P	35. Kt to K4	R to B4 (*k*)
17. BP × P (*g*)	R to Q2	36. Kt × KtP	Kt × Kt
18. Kt to K2	Kt to Qsq	37. R × Kt(ch)	R × R
(45m.)	(29m.)	38. P × R	K to B2
19. Kt to B4	Q to R3	39. K to B2	P to R4
20. QR to Qsq	P to B3	40. P to QKt3	Resigns (*l*)
		(2h.)	(1h. 40m.)

(*a*) Castling, instead of the text move, is recommended by the German masters.

(*b*) Is often played, but never previously by Mr. Blackburne.

(*c*) Best.

(*d*) Castling on the Queen's side would have been dangerous, owing to Black's open QB file.

(*e*) To keep Black's QKt on QB3.

(*f*) A good move.

(*g*) QP taking would have been the better move.

(*h*) P to Kt4 is far the better move.

(*i*) Here K to Kt2 ought to have been played by Black; the text move was quite an unnecessary venture.

(*j*) R to Q2 would still have saved the game.

(*k*) Kt to B5 might have given Mr. Gunsberg more chances to draw.

(*l*) The loss of the game, owing to the two passed Pawns, is now inevitable, the Black King being unable to approach either of them.

GAME III.—(GIUOCO PIANO.)

WHITE. Gunsberg.	BLACK. Blackburne.	WHITE. Gunsberg.	BLACK. Blackburne.
1. P to K4	P to K4	21. Q to K2 (*h*)	P to QKt3
2. Kt to KB3	Kt to QB3	22. P to KB3 (*i*)	Kt to Kt2
3. B to B4	B to B4	(46m.)	(1h. 21m.)
4. P to Q3	Kt to B3	23. Q to R6 (*j*)	QR to Ktsq
5. B to K3	B to Kt3	24. Q × P	Kt to Q2
6. Kt to B3 (*a*)	P to Q3	25. P to QB4	KKt to B4 (*k*)
7. Q to K2 (*b*)	Castles (*c*)	26. R to R3 (*l*)	KR to Kt sq
8. B to KKt5	B to R4 (*d*)	27. B to B2	Q to Qsq (*m*)
9. Castles KR	B × Kt	(1h.)	(1h. 53m.)
(10m.)	(25m.)	28. P to B4 (*n*)	QR to Rsq (*o*)
10. P × B	P to KR3	29. Q × R	Q × Q
11. B to R4	Q to K2	30. R × Q	R × R
12. QR to K sq (*e*)	B to K3	31. B × Kt	Kt × B
13. P to Q4	B × B	32. P × KP	P × P
14. Q × B	QR to Ksq	33. P to Q6 (*p*)	P × P
15. R to K3	K to R2	34. Kt × P	R × P
16. P to Q5 (*f*)	Kt to QR4	35. R × P (ch)	K to Kt3
17. Q to Q3	R to KKtsq	36. R to B2	R to R8 (ch)
18. Kt to Q4	P to KKt4 (*g*)	(1h. 26m.)	(1h. 58m.)
(25m.)	(59m.)	37. R to Bsq	R × R (*q*)
19. Kt to B5	Q to Bsq	38. K × R	K to B3
	(1h.)	(1h. 30m.)	(2h.)
20. B to Kt3	R to Kt3	Agreed to draw.	

(*a*) Blackburne usually plays QKt to Q2, or P to B3.

(*b*) Q to Q2 deserves consideration.

(*c*) P to KR3 would possibly have avoided some immediate trouble.

(*d*) Necessary. If Black plays, 8 . . . B to K3, 9Kt to Q5, B × Kt, 10B × B.

(*e*) To prepare for P to Q4.

(*f*) B × Kt would have been much better, for if P retakes Kt to R4 and B5, but, if Q takes, P to Q5, winning the BP.

(*g*) Black dare not take, for, if P × Kt, P to K5 (dis. ch.) wins.

(*h*) P to B3 at once would have been better.

(*i*) If P to R4, Black answers P to Kt5.

(*j*) The endeavour to gain RP loses too much time.

(*k*) Threatening to win the Q.

(*l*) Q to R3 would have been much better, as it would have avoided all subsequent complications.

(*m*) Intending Kt to R4 and R to QRsq.

(*n*) The only move, for if B × Kt instead Kt retakes, followed by R to Kt2, and wins the Queen.

(*o*) White threatens R to R3, retiring his Q to R3. Nevertheless Black might have played Kt to R4, for, if White plays R to R3, he should play R to Kt3, and it is difficult to see how White's Q could have been saved.

(*p*) All these moves require a great deal of exactitude. Black must play P × P, although White's Kt gets into a strong position in consequence.

(*q*) With the safe purpose of drawing.

GAME IV.—(RUY LOPEZ.)

WHITE. Blackburne.	BLACK. Gunsberg.	WHITE. Blackburne.	BLACK. Gunsberg.
1. P to K4	P to K4	21. P to QKt3	QR to Ksq
2. Kt to KB3	Kt to QB3	22. Kt to Q3	P to QKt3
3. B to Kt5	Kt to B3	(1h.)	
4. Castles	Kt × P (*a*)	23. Kt to Kt2 (*d*)	Kt to Kt4 (*e*)
5. P to Q4 (*b*)	B to K2	24. P to QR4	Kt to B2
6. R to Ksq	Kt to Q3	25. Kt to Q3	B to Kt3 (*f*)
7. B × Kt	QP × P	26. Kt to KB4	Q to Q3
8. Kt × P (*c*)	Castles	27. P to Kt3	Q to Q2 (*g*) (*h*)
9. P to QB3	P to B3	(1h. 26m.)	(1h. 14m.)
(20m.)	(13m.)	28. P to R4 (*i*)	B to B2
10. Kt to Q3	Kt to B2	29. P to KKt4 (*j*)	Q to Q3 (*k*)
11. B to B4	B to Q3	30. Kt to B5 (*l*)	R × R
12. Kt to Q2	B to KB4	31. R × R	R × R
13. B × B	P × B	32. Kt × R (*m*)	Q to Q2
14. KKt to B4	Q to Q2	33. Kt to B4	P to KR4 (*n*)
15. Kt to Bsq	P to Q4	34. Kt to K3 (*o*)	P × P
16. Kt to K3	KR to Ksq	35. P × P	Q to K2 (*p*)
17. P to B3	Kt to Q3	36. Kt to B5	Q to K5 (*q*)
18. Q to Q2	B to K3	(2h.)	(1h. 47m.)
(45m.)	(27m.)	37. Q to K3	Q × Q
19. R to K2	B to B2	38. Kt × Q	Agree to draw
20. QR to Ksq	R to K2	(2h. 1m.)	(1h. 52m.)

(*a*) The Berlin defence.

(*b*) Steinitz, in his game with Zukertort, here innovated R to Ksq, followed by Kt × P and B to Q3.

(*c*) If White plays P × P, Black's answer will be Kt to B4, when the exchange of Q's would not give any advantage to White.

(*d*) All this subtle manœuvring is for the purpose of acquiring advantages in position, and to avoid a draw by exchanges.

(*e*) To compel White's 24th move.

(*f*) Trying to exchange B for Kt.

(*g*) The position now is of such a nature that Black dare not attempt any advance.

(*h*) Both players think P to QB4 too risky.

(*i*) A very promising line of play.

(*j*) P to R5 deserves consideration.

(*k*) This stops White's threatening advance.

(*l*) If White played QKt to Kt2, Black might reply Q to Kt 6.

(*m*) White dare not take Q.

(*n*) This timely move assists in stopping what there might be left of White's attack.

(*o*) If Q to KKt2, White had a little more chance.

(*p*) To get the open file and prevent P to Kt5.

(*q*) It would not have been safe to leave Black's Q unmolested.

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